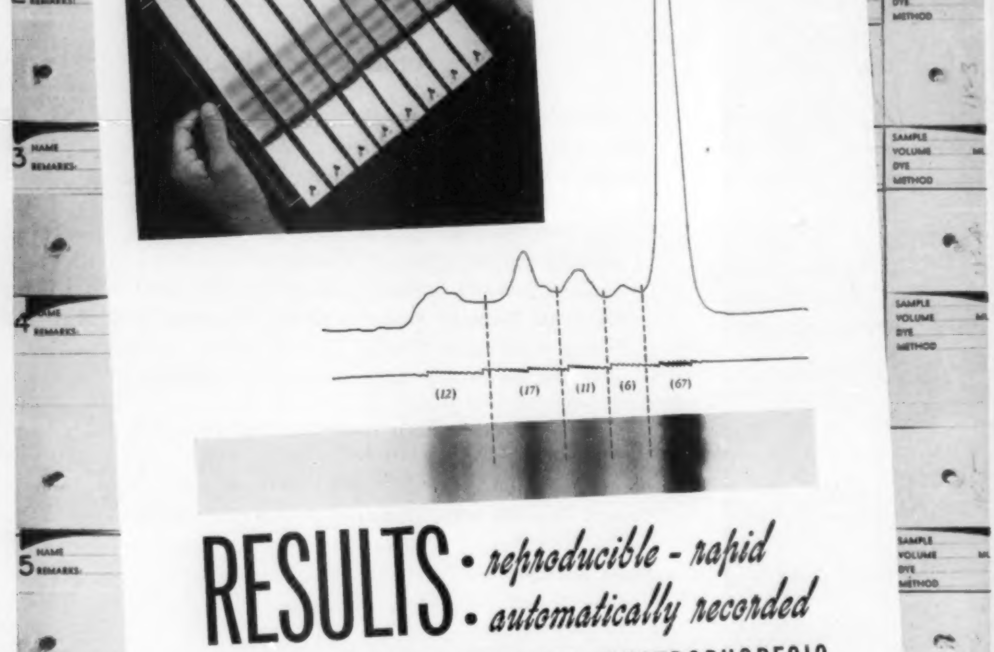
SCIENCE

15 APRIL 1955

VOLUME 121 NUMBER 3146

Annual Book Issue

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE


(12)
(17)
(11)
(6)
(67)
RESULTS • reproducible - rapid
• automatically recorded
NEW SPINCO Model R PAPER ELECTROPHORESIS
For the first time, a complete paper electrophoresis system with coordinated features for simple routine analyses, in the clinical laboratory. Every step is systematized from precision application of the specimen to final automatic recording of the relative concentrations. Ask for full details.
SPECIALIZED INSTRUMENTS CORPORATION
BELMONT 5, CALIFORNIA
spinco
Instruments and Supplies Stocked by Distributors in Principal Cities throughout U. S. and Canada
NAME
REMARKS
SAMPLE
VOLUME
DYE
METHOD
CHART NO.

Our Splintered Learning and the Status of Scientists

Conway Zirkle

Botanical Laboratory, University of Pennsylvania, Philadelphia

FEW scientists or scholars realize the sheer volume of our present-day publications. Not so long ago, the editor of one of our great abstracting journals stated that about half a million scientific papers were published each year. We can never know the exact number, of course, for some of them are printed in odd and out-of-the-way journals, and they range all the way from the truly scientific to the trivial. Also, we have no criterion that enables us to decide just when a paper is scientific and when it should not be included in the total. Even if we discard as many as one-half, those we retain would still present an intimidating volume. We can visualize their numbers, perhaps, by noting that during the time it would take to deliver a talk the length of this paper*, some fifty papers would be published. Obviously, we cannot read this paper and 50 others at the same time; no matter what we do, we will miss something. Nor can we ever catch up, because, between dinner one day and breakfast the next morning, another 600 papers will appear. Sometime during the annual meeting of the AAAS, a newspaper reporter had some good clean fun by emphasizing the number of papers the scientists read—and, of course, had to listen to. But during that meeting fewer papers were presented than were published elsewhere.

Our situation is clearly not as desperate as these facts and figures might indicate because, if it were, science would have been suffocated by its own products. After a fashion we have solved the problem of keeping up with ourselves, but the solution has not come cheaply. In fact, we have paid—and will continue to pay—a very high price for a workable method of utilizing the contributions made by our colleagues. Mercifully, perhaps, we do not realize the full cost but, whatever the cost, pay it we must. The solution, as we all know, is reached by dividing up the task, by our becoming specialists, and this means, of course, limiting our individual competence and interests. Now the modern specialist is not one who knows more and more about less and less, in spite of the oft-repeated libel. True, his field is continually narrowing, but the effective specialist who keeps abreast of the times must continually learn more and more about more and more. He must learn to the very limit of his capacity, because the sum total of our knowledge in all-important disciplines is increasing at an unprecedented rate, and so far it shows no symptoms of slowing down. If the specialist keeps up in his own field and understands what his neighbors are doing in adjacent fields—and all of this is necessary if science is to advance—he will clearly have little time or training to investigate or understand what is going on in more distant pastures. Thus, the price the specialist has to pay for his professional competence is often an all-encompassing and, sometimes, a very startling innocence.

The evil consequences of our fragmentary, and hence incomplete, knowledge affect both the scientists and the public at large. Perhaps the result easiest to identify is the slowing down of the progress of science itself. When the data necessary for a scientific advance are scattered and the logical connections between the individual pieces are obscured, the potential advance is simply not made. Important progress —progress in basic theory—is possible only when the raw facts can be collected and organized; only when they can be brought together into some one receptive mind. As long as they remain scattered in the minds of different specialists, the theory remains undiscovered.

Such conditions have existed, of course, from the very beginnings of science—long before our present division of labor, but, as specialization advances, the conditions worsen. Historians of science have recorded any number of instances where all the facts necessary for the birth of an important theory have been known for many years, but, as long as they were scattered, they seemed to be, and actually were, unimportant.

The great contribution of Gregor Mendel may be used to illustrate this point. His discoveries were presented to the world in 1865, but the biologists who read his paper thought nothing of it. Thirty-five years later, in 1900, the paper was discovered and appreciated, and the new science of genetics was born. The original neglect of Mendel's work is truly startling when we realize that every single one of his discoveries had been made previously and that some of them were well known.

In 1822, the very year in which Mendel was born, two horticulturists, Seton and Goss, working independently, announced most of the facts we label Mendelian. They described dominance and recessiveness and the segregation of these types both in the second hybrid generation and when the hybrid was bred back to the recessive parent. They announced the existence of two kinds of dominants—those that breed true (homozygous) and those that continue to segregate (heterozygous). When Mendel was 4 years old, Sag-

* Based on a Phi Beta Kappa address given on 27 December 1954 at the Berkeley meeting of the American Association for the Advancement of Science.

eret described the independent assortment of what we call Mendelian factors. Indeed, every bit of Mendelism, except the definite ratios, was well known to the plant hybridizers, and in 1854, even the definite ratio was discovered by Dzierzon—in honey bees. It is probably more than a coincidence that Mendel, the man who created Mendelism, both hybridized plants and bred honey bees, and, as far as we know, he was the only man at the time who did. At any rate, the scattered fragments of Mendelism came together for the first time in his mind, and 35 years after he published his classic paper, the importance of his synthesis was finally recognized.

Such examples as Mendel could be cited indefinitely. In 1813, when Charles Darwin was 4 years old, William Charles Wells described evolution by means of natural selection in a work entitled "Two essays, one upon single vision with two eyes, the other on dew, and an account of a female of the white race of mankind part of whose skin resembles a negro! with some observations on the causes of the difference in colour and form between the white and negro races of men," and in 1831, Patrick Matthews did the same thing in a book entitled *Naval Timber and Arboriculture*. Darwin did not know of either work until after he had published *The Origin of Species* in 1859. Whereupon he wondered rather plaintively if he should have explored the field of growing timber for the navy.

Today we have no way of knowing how many great scientific discoveries have already been made—made, but never assembled—discoveries that are hidden because they exist only as scattered fragments. The past is full of such instances. We know now that the raw data for evolution itself were available in classical times. Then species were thought of as being unstable, as being constantly changing units. Also, belief in the inheritance of acquired characters was held almost universally, and a few philosophers even used natural selection to explain the existence of adaptation. The "fact" of changing species, however, never got together with any of its possible explanations. But we can spend no more time on this aspect of our scattered knowledge. It is an inherent limitation of finite minds and will continue to exist in the best of all possible worlds.

Of much more importance are the effects of specialization on the minds of the specialists themselves. These effects are not always happy, but we would be both supercilious and ungrateful to place the blame on the scientists who specialize, on those who concentrate their interests so intensely that they are able to advance the frontiers of knowledge. Without specialization, without the division of intellectual labor, we would have difficulty in living as civilized beings. All of us owe a debt to the scientific specialists, and it is only sporting to give them the respect they deserve. It is silly to allow an occasional lapse of good sense in a minute fraction of the scientists to affect the status of all scientists. This point cannot be emphasized too strongly, for today the American scientists are on the defensive and desperately need all the prestige that is their due—the prestige usually accorded to scientists in other countries.

Scientists now have to deal personally with politicians and with military men. They are not in too strong a position, as many of them have discovered. If scientists can be presented to the public as well-meaning individuals who, by some miracle of nature, have developed into a strange combination of magician and crackpot, and if this caricature becomes the accepted stereotype, the scientists will not be able to protect their interests or maintain conditions in which science can thrive. Scientists, for example, are vitally interested in the loyalty tests and would like to introduce more common sense into the clearance procedures. They would also like to be able to hold international scientific conferences in the United States, but so far they have been too weak to achieve either of these aims. A single Klaus Fuchs or Bruno Pontecorvo, or even a single starry-eyed scientist who makes the headlines from coast to coast, may dissipate more prestige than the scientists can afford to lose.

Then, too, there is the ever-present danger of anti-intellectualism, and anti-intellectualism is a disorder that has accompanied our species throughout its entire history. Socrates felt its full impact. It finally dominated the classical world, and conformity reigned in all cultural fields. For 1000 years, beginning about the 4th century of our era, anti-intellectualism reduced the progress of science to a snail-like crawl. Today, it is stirring restlessly and at any time it may again become virulent. It has already destroyed certain sciences in the Communist world, and it threatens freedom everywhere. We will have to check and control anti-intellectualism if we are to maintain our progress. But scientists often stimulate its growth through sheer naïveté.

If scientists are to fight anti-intellectualism effectively, they must look to their armament; they must check and perhaps improve their equipment. Certainly they should know why they are on the defensive and why they are vulnerable. Several times I have heard scientists state that "we have not done a good job in educating the public." It is possible also that they have not done a good job in educating themselves and that it is their own educational deficiencies that are the immediate cause of their lowered prestige.

Every now and then someone discovers that a large proportion of our better Ph.D.'s are uneducated. The discovery is generally announced under humorous or semihumorous circumstances and everyone enjoys the occasion. An editor or two wonders what the world is coming to, those who are not Ph.D.'s feel complacent, and the occasion passes. Yet the question arises: What else should we expect? When we consider what the Ph.D.'s have to go through, how could it be otherwise? True, some centuries ago the doctor of philosophy was a learned man, a philosopher, fairly well acquainted with all that was known, but today, such competence is beyond the capacity of anyone. Historically, the doctor of philosophy was a scholar, and also a man who was more than a dilettante in the sciences. He

was a man who had assimilated that great achievement of our species known as the humanities. During the Renaissance, the scientists were the humanists, and they were humanists because otherwise they could not be scientists. But these conditions no longer exist. If we glance at our educational system, we can easily understand why.

A great part of the freshman year in college, if not all of it, is devoted to learning what the students should have learned in high school, but did not. One year later—at the end of the sophomore year—the student "concentrates his efforts in the field of his greatest interest," and this concentration continues. In graduate school the training will be narrowed even more, and here the student really has to devote his time to acquiring the accumulated knowledge in his field and to mastering the techniques of research and the other holy mysteries of his profession. He also has to eat and sleep and sometimes teach his subject to undergraduates. Even equipped with the enormous energy of the young, he has little time or attention to devote to side issues, to issues that will not help him earn a living or promote him professionally.

Recently, H. J. Fuller [*Science* **120**, 546 (1954)] of the University of Illinois asked 15 candidates for the doctorate in philosophy to identify the Renaissance, the Reformation, the Monroe Doctrine, Voltaire, the Koran, Plato, the Medici family, the Treaty of Versailles, Bismarck, and the Magna Carta. The identifications were satisfactory just 35 percent of the time. Fuller has recorded all the startling details of his experiment, and they are well worth reading. The questions he asked, of course, were completely "unfair" to the students, since they were earnest youths who had been caught in our system of education and were only trying to become doctors of philosophy in one of the botanical sciences. They also have to earn a living in a competitive field; and it is possible that, for this purpose, nothing better than a doctorate in philosophy was available. So they became Ph.D.'s! We are reminded of the famous scene where Dr. Watson discovers that Sherlock Holmes had never heard of the Copernican theory. The good doctor describes it to the great detective, but becomes goggle-eyed when Sherlock states that now that he has learned it, he will forget it as soon as possible since it has no bearing on his profession—the identification of criminals.

We are now faced with the question: How should Ph.D.'s in science be educated? Or the even more fundamental question: Should they be educated at all? There are many considerations. Whichever course we pursue will have both its advantages and disadvantages. There is no easy answer. For example, what effects will education have on a scientist's output? There is no doubt that, no matter how brilliant a scientist may be, he should also be industrious. It is not enough for a man to be a scientist, he must also work. Now, perhaps, the greatest single stimulus for industriousness lies in the ability of a scientist to overestimate the importance of what he is doing. This ability, of course, varies inversely with the breadth of his education.

An argument might be made against educating scientists—if any one could be found who would be cad enough to make it. It might also be urged that the scientist, as an individual, would suffer little through a lack of education. No matter how narrowly his training was channeled—provided it was not so limited as to injure his research—the scientist could still be a happy citizen—provided, of course, that he did not stray so far afield as to become lost or to put his name on too many petitions. Nor would he suffer socially, so long as he had colleagues with whom he could talk shop. He need never be bored, because even the more specialized fields contain enough material to occupy any stray thoughts that might wander into his mind at odd moments. It is doubtful if he would even feel a sense of loss, a sense of having missed something important. Finally he could experience the thrill of being respected by those who understood his work. Certainly he need not be patronized by anyone who has never mastered any field or even by a college graduate who has read 100 books. It is worth noting parenthetically that one does not have to be a specialist to have a defective education.

But, collectively, scientists suffer because of their lack of education, and society suffers even more. As things are today, scientists would be better off if they enjoyed more public esteem. Scientists do not thrive when anti-intellectualism grows, but, instead of diminishing this blight, many scientists have unwittingly helped it along. For the last two decades, far too many scientists have had too high a *G.Q.*, or gullibility quotient. When the gullibility of one scientist becomes public knowledge, all scientists suffer. I can sympathize with one of my learned friends who said that he had a divine right to make a fool of himself whenever he wanted to, and that he would not let anyone stop him. This is an important right, of course, and we should not surrender it lightly. More of our fundamental freedom rests upon it than appears on the surface. The right to make honest mistakes is basic to all progress, but if we exercise the right to excess, the more unsophisticated of our fellow citizens will misunderstand. We should remember that the right of a scientist to make mistakes does not include a right to indulge in slovenly or escapist thinking. A few years ago a C.I.O. official said to me, "We in the C.I.O. caught on to the Commies long ago, you college boys were the suckers." The truth is that most of "us boys" were not suckers, but the sad fact is that some of us were, and they were the ones who made the headlines.

In spite of much adverse publicity, we know that only a very minute fraction of the scientists have been disloyal—and that fraction is, in fact, vanishingly small. The number of good scientists in the free world who are Communists or who follow the Communist line in science can be counted, perhaps, on the fingers of one hand. If we think for a moment, we can readily understand why first-rate scientists do not become

Communists. Scientists do not like to be disciplined, and few would tolerate the rigid intellectual and political discipline of the Communist Party. Scientists like to talk about their interests, but the party does not approve of garrulity or of men who are independent or who show individual initiative. Our scientists are even chafed by and resent having their work classified, and, by comparison, this involves the mildest of possible disciplines. Certainly the party would never trust honest scientists too far. After all, scientists have to be completely honest in their work, and this honesty tends to become a state of mind. Thus, scientists are really very poor material for any conspiratorial organization. Outside of the party, however, and without too many contacts with practicing Communists, the scientists' very virtues may dupe them. To the pure, unfortunately, all things seem pure, and scientists are remarkably pure in thought and can be innocent in more than one sense. But, in spite of this, the percentage of scientists who were duped by "our gallant ally" was probably just about the same as that of the whole population. After all, in spite of his concentrated interest, a good scientist must have a certain amount of native intelligence, and when he is well informed, he tends to think clearly.

When we examine the whole picture and balance all the conflicting factors, we can probably agree that even specialized scientists would profit by an education. Their lives would be richer and their standing in the community enhanced. They should not wander too far into the fields of scholarship, however, as time is fleeting and they have a job to do. Perhaps they could adopt scholarship as a hobby, provided they did not let the hobby get out of hand or select some one small aspect of learning and make it into a secondary specialty. We may agree that we would all be better off if our scientists were truly educated, but the method of accomplishing this is, at present, not at all clear.

Meanwhile, we should protect our scientists both from the anti-intellectuals and from themselves. Some temporary expedient may prove valuable. Perhaps our scientists could learn caution and learn to evaluate their training realistically—to recognize their academic degrees for what they are really worth. A rewording of the diplomas might help in the reorientation. The completely imaginary certificate shown here will illustrate the point. The wording, of course, may be altered to suit the occasion. This is a very tentative suggestion.

The Johns Hopkins University
certifies that
John Wentworth Doe
does *not* know anything but
Biochemistry.

Please pay no attention to any pronouncement he may make on any other subject, particularly when he joins with others of his kind to save the world from something or other.

However, he worked hard for this degree and is potentially a most valuable citizen. Please treat him kindly.

Such a diploma might have a healthy psychological effect upon its recipient. It might serve as a warning to him not to rush into complicated problems with some innocent and naïve solution. This is important, for the fewer the scientists who are caught off base, the less their prestige is lowered.

Obviously the specialized scientist pays a very high price for his professional competence. He has compensations, however, and his pursuit of happiness need not be too greatly handicapped. But the price that society pays for the unintegrated state of its knowledge is much higher, and society has no compensations whatever. I do not mean to imply that society will meet disaster unless the millions of facts recorded in the millions of scientific contributions are organized and made available to all. The lack of integration is on a much more fundamental plane. Even the basic concepts and verified generalizations of science are scattered, and many obstacles other than their number and complexity stand in the way of their proper integration. Much scientific knowledge is hostile to some of our best-loved oversimplifications, and this knowledge, of course, will not be welcomed. When unwelcomed facts are scattered, they are much easier to avoid since we can deflect our minds from them at the first hint of their presence and before we have to face the implication of their meaning. For a long time we have had well-tested ways of disposing of facts and ideas we do not like, and we do not hesitate to use them. A partisan mind has undoubtedly been standard human equipment for the last million years. It is, and probably always has been, standard mammalian equipment. The partisan mind is one of the most effective of all isolating mechanisms. It establishes the vicious, little personal censorships that segregate us into groups and keep our information scattered—such censorships that keep us from reading certain books and periodicals that present unwelcomed facts.

Often the individual oversimplifications, which mean so much to us personally, were acquired in adolescence or even in preadolescence. To alter them we might have to rearrange our neural circuits, and this might even result in ulcers or in a nervous breakdown. It would certainly be painful. There are no limits to the examples we might cite of this craving to avoid the complex and the puzzling and to live in a simple "yes-no" universe. In almost every field we tend to classify the actors into "the good guys and the bad guys." In politics especially! It is really amusing to contrast the clear, logical, and accurate way we evaluate the campaign speeches of the politicians of the *other* party, with the semiconscious euphoria we exhibit when listening to the oratory that emerges from our own party, the party of the "good guys." We are able to apply even an ethical or an orthodox test to scientific hypotheses: some are forward-looking and virtuous, others are evidence of sin. Thus, a complete integration of all human knowledge, which, incidentally, is impossible, would involve a great deal of re-education, and re-education is always more painful than education itself since it involves unlearning

as well as learning. Officially we are all in favor of the truth, no matter how disconcerting it might be, but we do not feel the need of going around looking for trouble. Perhaps, without admitting it, we are convinced that the truth that, proverbially, will make us free, will, at least temporarily, make us unhappy.

It is hardly feasible to list all the impediments to a proper integration of human knowledge. We have become so accustomed to viewing the universe in splintered bits that many of us really assume that it has a cellular structure and that each cell can be treated conveniently as if it were a pigeonhole. This view is widespread even if it is not held overtly. It is the view that college and university administrators seem to favor, for it promises to simplify their always-too-complex problems. Whenever they can, they assign a single pigeonhole to the custody of the corresponding academic department. Thus, by increasing the number of departments, the larger colleges and universities may, literally, cover the universe, neatly, completely, and without jurisdictional conflicts. And each savant on the faculty will know just where he stands. Well, the concept at least is orderly!

Fortunately, in the physical and natural sciences, the partitions between pigeonholes are becoming very permeable. Ideas are percolating, and the scientists themselves are beginning to wander about and to explore adjoining compartments. Many a specialist finds himself working in two or more compartments and, often before he knows it, finds himself able to communicate with two or even three different groups of natives. These scientific explorers make excellent liaison officers. Although as individual scientists they retain the usual human limitations, they are beginning to tie the sciences together, and, professionally at least, they rarely wander far enough to get lost. Neighboring pigeonholes are being welded together even if they are not coalescing, and we can be grateful for this. There is still no prospect of unifying distant compartments, however, or of discovering the proper system for organizing the information that is widely dispersed.

Some partitions between the pigeonholes serve as real isolating mechanisms. In spite of the fact that the scientists and the humanists were originally the same individuals, today they have few points of intellectual contact, for their professional interests are now too far apart. The case is not hopeless, however, because they do have some personal and social contacts and they even seem to admire each other as individuals. They now serve together on college committees, compete for their shares of the academic budget, and, occasionally, unite to oppose some bright scheme of the administration or of the athletic department. They are natural allies, and if only they had the time, each could understand and respect what the other is doing.

The partitions separating the biological and the social sciences are also nearly impervious, and intellectual contacts between the disciplines are difficult and uncertain.

I have recently had the task of tracing the history of the Communist line in biology, the line that was drawn by Marx and Engels in the 1870's. It was the resurgence of this archaic biology that destroyed genetics in the Communist world, and this biology also sets the standards in much of our own culture. It permeates our belles lettres and runs right through the social disciplines. It affects the thinking of many people who are unaware of its origin or of its implications. Nowhere else are the evil consequences of our scattered data more obvious or more crippling than when an archaic quackery is interposed between the separated splinters of learning and used to muddle the thinking on which our welfare depends. The continued existence of this Marxian biology is possible only in those fields that are isolated from biology proper; it is possible only where communication is defective. Biology is found in the elementary textbooks of sociology and, of course, will be found there as long as *Homo sapiens* remains a mammal. It is not the biology of the biologists, however, nor, fortunately, is it the biology of Marx and Engels. It can be described only as sociological biology. It is a law unto itself, although it overlaps the other two biologies—the Marxian perhaps more than the biology of the biology departments. There is not much we can do about this at present. As long as our learning is splintered, such anomalies as sociological biology will arise.

As individual scientists and scholars, all that we can do is to make the best of our personal limitations and, within our limitations, to lead lives as useful and happy as we can make them. After all, we will not live very long. But we are also members of society and we are citizens in a state, and as citizens we, or at least those we choose as leaders, will have to make decisions. If some of the decisions are uninformed and based on faulty data, we may expect them to be expensive, may expect their cost to run to 10 or 11 figures—not to mention the cost that cannot be expressed in money. Perhaps all the data we need for wise decisions are not in existence, but in the past we obviously did not use all the information we had.

The unavailability of our scattered information has seriously reduced the efficacy of our thinking on the national level. In retrospect, many of our collective decisions seem uninformed and even naïve. We need not go very far back in our history for illustrations—no further than to the war that ended all war and made the world safe for democracy. We are unanimous in not wanting war, and our reactions are reasonable. To achieve our desires—to abolish war and make the world better—we have only to discover the causes of war and remove them—or refuse to fight or do something.

During World War I, the majority opinion seems to have been that the war had come upon us through the fact that the Kaiser, the Crown Prince, and the German army wanted war. We succeeded in removing these causes, but after the Treaty of Versailles, things looked different. Then we were led to believe that we had been involved because of the machinations of the international bankers. These bankers were bad men

who had loaned money to the Allies and then tricked us into saving their investments when the Allies were threatened with defeat. This notion was actually endorsed by our government, and Congress passed a law forbidding our banks to lend money to nations at war. The policy we adopted was known as "cash and carry." This policy, however, lacked something, and after World War II started we swapped it for "lend-lease," and in so doing made one of the neatest flip-flops in our history. We still hated "foreign wars," but we would become the "arsenal of democracy." However, Pearl Harbor changed all this. Incidentally, we are definitely showing signs of maturity and are no longer seriously trying to blame Pearl Harbor on a single individual. But, in retrospect, we know that before the debacle at Pearl Harbor we had all the facts that could have prevented the surprise had they been organized and their significance understood.

In the last few years we have begun to suspect that our negotiations at Teheran and Yalta were not only uninformed, but were in part actually misinformed. We do not yet know all that happened in these negotiations, but we are beginning to suspect that our decisions did not represent the ultimate in foresight or wisdom. Our chief representatives were undoubtedly sincere, but even able diplomats will be overly handicapped if they are uninformed on basic theory. No matter how wise an individual may be, he is bound to err if he has to rely on inadequate or faulty data. Although our ignorance of Russia seems to have been a trifle excessive at Teheran and Yalta, we are now judging the effects in the light of our hindsight. Today we are Monday morning quarterbacks and are not forced to make decisions under pressure. But this does not alter the probability that, had our negotiators been better informed, we would be better off today. The less we say about the loss of China to the free world, the better.

All our mistakes, of course, are not in the past. We are certainly making mistakes now and will do so in the future, but some time will have to elapse before we can learn what we are doing that is wrong. Our need for accurate information is pressing. Any number of questions are presenting themselves, and our welfare will depend upon the answers we obtain. What are the important facts we are now overlooking? Obviously, we do not know, nor can we expect even an I.B.M. machine to organize our data and give us the answers we need. Basic theory is involved and, of course, this always impinges on our most sacred beliefs and on our emotional irrationality. Needless to say, political predilections will obscure much of the picture. We have to act, however, and take calculated risks, knowing that we have all too little upon which to base our calculations.

We will indeed make mistakes, and when we look back on our present actions, we will undoubtedly find that many of our errors of judgment were unnecessary. Fortunately, most of our individual errors are trivial, but a major danger exists if our errors become synchronized. Disaster will threaten our way of life if a majority of us are fooled in the same way and at approximately the same time. But, even if this should happen, a vigorous minority functioning as a negative feedback may give us sufficient stability so that we can recover from even the most popular aberration. Incidentally, we have some horrible examples in what happened to several great and learned nations when they succumbed to an aberration and would not let their negative feedback work—when all minorities were silenced or liquidated. These nations were unable to hold to a rational course; they fell into totalitarianism and, once there, they stuck.

But we can take comfort in the fact that we are not the only ones who appear ignorant in retrospect. Up until now, and fortunately for us, our enemies have blundered more than we have. Hitler's many idiocies are now a matter of record. Stalin's application of Marxian biology to Russian agriculture was stupid and uninformed, but very lucky for us. It is, perhaps, the greatest single deterrent to World War III. At present the Russians simply do not have enough spare food to allow them to take major risks.

To err is certainly human, but we would like to reduce the incidence of error in our society. If international tensions mount and the cold war continues to be waged on many fronts, victory might well go to the side that is less confused intellectually, to the side that is less scatterbrained. Today our nation definitely needs all its brain power, and this means that it needs the help of our scientists not only as creators of military gadgets, but as citizens who possess unique and valuable information. If our scientists could truly pool their knowledge, if the pool could be organized into a whole that could function undisturbed by the partisan limitations of individuals, and if the scientists, politicians, and military men could learn to speak the same language, we would go well ahead of the Communist world, and victory in the cold war would be assured. Before we can reach this desideratum we will have to learn how to pierce the isolating partitions that dissect the world of learning and to remove the artificial impediments to communication. We will also have to establish an atmosphere in which good and loyal scientists may function effectively, unhindered by bureaucratic formulas or by demagogic attacks.

We can summarize the effect of our splintered learning briefly. The history of science contains many instances where facts, which could have led to major advances, were so scattered that the advances were not made. Progress had to wait until the facts were rediscovered in a context where their significance was understood. This slows up the progress of science, but its ill effects are not irreparable, for science still manages to advance with ever-increasing speed. Far more important are the effects of the fragmented learning on the scientists themselves. The limited interests and the lack of background of a few scientists do affect their behavior, and these few lower the standing and prestige of all scientists. This is serious because, in dealing with the political authorities and with the mili-

tary, the scientists need prestige and respect. As it is, the scientists are not in a position to lead from strength. They are not even strong enough to look after their own proper interest or to combat effectively the anti-intellectualism that is ever present. The status of science can be lowered by a single naïve scientist in spite of the unprecedented accomplishments and contributions of science. Also, the general repute of scientists can suffer from the public activity of any small group that believes that all scientists should be supported automatically, and that whatever a good scientist does is good. Scientists would be in a much stronger position if they had the respect that society generally gives to the "practical" man or even accords to the gentleman and scholar who is a judge of the finer things of life.

In many of the sciences, the Ph.D. is a vocational degree, a preliminary step in getting a job. The acquisition of the degree, however, is no light task. It takes a minimum of five years away from the education of the candidates and devotes the time to their professional training. In spite of their native intelligence, many scientists show the effects of this sacrifice and, when they wander too far from the fields they know, they get lost.

Society also suffers from its inability to utilize fully the data that are now accumulating so rapidly. Decisions on the national level frequently have to be made suddenly, and those who act on the higher levels have to take calculated risks. Practically no individual is equipped for such a task, and we have learned to substitute small groups for individuals when crucial decisions have to be made—such groups as a cabinet or general staff, or even a research team. But all too often, when fundamental theory is involved, serious gaps of information appear in the collective knowledge of the group. Sometimes the knowledge that could fill these gaps is simply lost in the vast fund of our undigested learning, sometimes it is excluded by partisan thinking or by the human desire to evade what is complicating. Whatever the cause, the effect has been an inability to focus all the relevant data on the questions that so vitally concern our national well-being. Errors of judgment, of course, are inevitable. True, we are often able to correct our past mistakes—we have a major opportunity every 4 years—and this ability may be our greatest source of strength. It may give us the adaptation that we need for survival in the world of today. All nations do not have this ability; for example, Hitler and Stalin could be removed only by death, and death does not always arrive when it can do the most good.

Today we are faced with a real struggle for existence, and it is not just a competition between individuals, but a contest between systems—between different ways of life. The fit, of course, are not those who do no wrong, but those who can learn more quickly by experience. We may take some comfort in the fact that the enemies of the free world also make errors, but they cannot correct their errors as easily as we can correct ours. Our ability to correct our mistakes gives us a very real advantage, and we would be silly to throw it away. Since we do not have our facts well enough in hand to escape even the avoidable errors, we must preserve our freedom to change our course of action—we must preserve enough freedom to give our hindsight a chance. Our chronic lack of foresight then need not be fatal.

The Use of Material*

Ralph E. Cleland

Department of Botany, Indiana University, Bloomington

LAST year *Biological Abstracts* printed references to 33,498 publications. As science has grown in extent and complexity, the volume of scientific publication has expanded to phenomenal proportions. We scientists are busy men, and when we are confronted with the flood of journals pouring into our libraries, journals containing hundreds of articles that we should read to keep abreast of our fields, we are sometimes tempted to throw up our hands in despair and give up the one-sided fight—the fight to add our own share to the flood. Whatever we do, it is a safe bet that none of us read all the papers we should read or even all the papers that their authors hoped we would read.

Now it is obvious that this deluge of literature is not presented to the scientific public without a purpose. The purpose, of course, is a mixed one. The ego is inflated when one sees one's self in print. Besides, one gains scientific standing by means of published contributions, to say nothing of promotion in academic rank when the number of titles becomes sufficiently multiplied. On the other hand, there is an altruistic motive behind all of this publication. Scientists as a group do not attempt to gain financially by control of their product. For the most part they tend to present their findings to their colleagues as a gift, and scientific writing is their medium for making this gift available. The purpose of scientific communication is therefore a compound of the desire to get ahead and the desire to make a contribution to the progress of science and civilization.

* Read at a symposium, The Communication of Research Results, at the annual meeting of the American Institute of Biological Sciences, Gainesville, Florida, 7 Sept. 1954.

Whatever the objective, however, it is clear that this goal will not be achieved if the published material is not read.

All of us have two major needs to be satisfied when we go to the literature. On the one hand, there is a relatively small number of papers that we must read in their entirety because of their importance in connection with our own research or teaching. On the other hand, there is a large number of papers, in our general field or in fields that are important to us in one way or another, with which we should become familiar in order to be aware of what is going on. We cannot possibly find the time to read all these papers carefully and thoroughly, nor are we sufficiently concerned or conversant with the work to require such detailed scrutiny. What we desire from them is their general conclusions, and we need to have them presented in such a way as to give a brief informative summary of the reasoning and findings. We therefore want two rather distinct things from the literature: (i) detailed evidence from a relatively few papers, and (ii) general summaries of a much larger group of publications.

To take care of these needs, the best method so far devised has been to publish most papers *in extenso,* thus providing for the needs of the specialist; and to include in each paper a summary which those less immediately concerned with the research can read. This is a very costly process, and it may be questioned whether it is the most efficient and practical system that could be devised. Take, for example, the case of a biological society that publishes annually a journal of 1000 pages, comprising some 100 separate articles. At $20 per page, a 10-page article costs $200 to publish. But this article is read in its entirety by perhaps not more than a score of persons. The society is spending $10 for every reader who reads this particular article from beginning to end. Multiply this by the number of articles published and ask yourself whether the results justify the expenditure.

The question I raise is this: Should an article that is read in its entirety by only a few persons be published as though it were to be thoroughly perused by many? Why give a circulation of 2000 to 3000 to an article that will be critically examined by only 20—or even by 100?

I wonder whether the time has not come for us to apply ourselves to the development of a more economical and practical system, one that will give the specialist access to the detailed information which he needs and at the same time relieve the individual scientist of the cost of publishing *in extenso* a multitude of papers that are of little or no interest to him.

In order to insure that our papers will be read by those who should read them, most of us in biology follow a practice of exchanging reprints on a rather extensive scale. In this way, in exchange for our own publications, we receive many or most of the articles of primary concern to us. Suppose a journal, instead of going through the costly process of publishing our papers in their entirety, required us to prepare for each paper a condensed version, omitting detailed data and documentation, and would accept for publication in the journal only this version, at the same time providing some other mechanism by which the full paper would be made available to those to whom we would ordinarily send reprints. Thus, we would depend upon some substitute for reprint distribution to get the fully documented paper into the hands of those who needed it, and the journal would be relieved of the necessity of printing the whole paper for the benefit of a relatively few persons. Is it possible to visualize a process by which this could be accomplished?

Two programs now in operation will illustrate the type of arrangement that I have in mind. First, there is the American Documentation Institute, which will prepare microfilm editions of full-length papers, shorter editions of which can then be published in journal form. Perhaps the facilities of the institute could be expanded in cooperation with the various journals, and perhaps a pattern could be developed whereby these journals published only condensed versions of papers, the full-length editions being available to all interested persons at minimum prices on microfilm. A second plan of this sort is also in operation, but at present it is limited to doctoral dissertations. University Microfilms, Inc., of Ann Arbor, Mich., now cooperates with a large number of the leading graduate schools of the country. The first copy of each thesis is sent to University Microfilms for microfilming. An abstract of the thesis accompanies the manuscript, and this is published in the journal *Dissertation Abstracts.* At the end of each abstract the price of a microfilm copy of the full dissertation is given.

A third possibility would be to effect a comparable arrangement using microcards. I, personally, would like to see microcards used rather than microfilm since they are easier to read, one can move from one page to another more readily, and cards are much easier to file than microfilm. With few exceptions, journal articles could be included on a single microcard, which can accommodate almost 50 pages. Even at 20 ct per card (and the price could be brought down if the scheme became widely adopted), it is probable that it would cost the average productive worker no more to buy microcard editions of the papers he wished to receive than to buy and distribute reprints of his own papers in the hope of receiving these papers in exchange. For example, I recently paid $35 for 400 reprints of a 15-page article. To this must be added the cost of mailing, an estimated $15. For this total of $50, I could buy, at 20 ct per card, 250 articles by other authors.

I have published about 50 scientific articles over the years. At present printing prices it would have cost me at least an average of $50 apiece to buy and distribute reprints of these articles. For this amount I could have bought 12,500 papers by other authors on microcards. My total reprint collection now includes about 14,000 items of which at least 2000 or 3000 are papers that I would not have bought and for which I

will probably never have use. Thus, I would be at least as well off financially today if I had bought reprints of other authors on microcard and had sent out no reprints. And I would be far better off insofar as shelf space is concerned.

The essence of these suggestions, then, is that journals cease to publish papers *in extenso,* and publish only digests of articles. The page limit would be reduced from 10 or 20 pages to one or two pages. At the same time, by cooperation with an organization prepared to manufacture microfilm or microcard editions, full-length editions of the papers would be available to those who wished them. Presumably, journals would still be willing to print longer papers if the authors were willing to pay the excess cost. In many cases, on the other hand, authors would probably find that they could say all they wanted to say within the reduced page limit, thus avoiding the necessity of writing two papers—a long and a short edition. The art of brevity would thus be cultivated more assiduously.

A plan of this sort would transfer from the author to the recipient the cost of placing fully documented and authenticated articles in the hands of experts. This is perhaps the fairer and more efficient way of handling the matter. A person would then buy what he needed instead of making gifts of his own publications and hoping that he would get what he needed from others as gifts in return. Furthermore, he would accumulate a library of papers wholly of his own choosing, instead of a library chosen for him by those who decided to favor him with gifts. I am sure that his library of articles would be considerably smaller than it now is; but it would be better chosen, it would include just what he wanted to have, and it would omit what he had no use for. An additional advantage to the investigator would lie in the space saved. This would be especially true if microcards were used. These would be filed in ordinary card files and would take less than 5 percent of the space that printed reprints of the same articles would occupy.

The reader's needs are not wholly met, however, when he has at his disposal journals containing digests of longer papers, with the longer papers available on microfilm or microcard. Often he runs across a reference to an article that may be important or not—he has no way of determining until he has seen it. Furthermore, he wishes to see the article at once and not wait for several days until he can get it on card or film. He needs what the university library now gives him—a rather complete coverage of the articles *in extenso.* Even this can be achieved with the proposed system. A journal which published only digests should be able to reduce its subscription price to libraries to such an extent that university and college libraries would be able to subscribe to the microfilm or microcard edition of the journal as well as to the journal itself. Thus the investigator could consult articles on card or film in the library, and if he later wished to purchase copies could do so.

This suggestion envisages the continued independent existence of the various journals and society organs. It might be fruitful, however, to look ahead a little farther into the future and visualize a possible additional step.

Most of our journals are issued by a small group of men, often only one or two, who are not trained journalists, who do the job on the side, often without compensation, on top of a full professional program. They are not particularly expert at the job and by the time they gain some competence as editors or business managers they resign and turn the work over to other novices. Under the circumstances the operation cannot be fully efficient. It is bound to be costly in time, energy, and money.

In contrast, the average newspaper is a vastly more efficient organization. For example, a weekday copy of the *New York Times* selected at random contained 141.2 columns of printed material, excluding advertising and illustrations. This material averaged 960 words per column, or about 135,834 words of news material. This is about twice as many words as would be published by the *American Journal of Botany* in an entire year if it published only digests. Yet this amount of material is written, assembled, edited, printed, and distributed every day, and much of it in a single day's time. Obviously the editorial and manufacturing staffs are huge, but the costs per word printed are low in comparison with what it costs to put out a scientific journal.

I have wondered whether we might look forward to the time when most of the biological journals in this country would join forces and go together to put out a single journal, which might take on something of the format of a tabloid newspaper or the magazine section of the Sunday *New York Times.* It would appear weekly and would be departmentalized, in a manner similar to *Biological Abstracts.* Each department could be under the editorial supervision of the society or organization now producing the journal in that field. Selection and review of articles would be under the direction of these editors. The journal would accept digests rather than extended articles and would provide microfilm or microcard editions of all papers for purchase by individuals and libraries. It would contain, in addition to these digests, editorials, symposiums, news items, letters and discussion, book reviews, and advertising. Individuals could build reprint files either by clipping articles and pasting them on standard size sheets, or by purchasing microfilms or microcards. It would no longer be necessary or possible for authors to buy reprints. The journal would presumably be printed on ordinary newspaper stock, and half tones would be handled as they are in newspapers—they would be adequate but not of deluxe quality.

The advantages of such an arrangement would be speed and economy. Speed would be achieved by the adoption of newspaper manufacturing techniques. For instance, such a journal could be printed on rotary presses that would cut the time of actual printing to about 10 percent of the time necessary when flat

presses are used. Economy would result from utilizing newspaper techniques and also from increased advertising appeal. If subscriptions were permitted only to the journal as a whole, and not to individual sections, advertising appeal would be increased to a maximum; the amount of advertising should then be such as to make possible a substantial reduction of subscription rates to individuals.

If such an arrangement could be made, one American biological journal would take the place of several now issued. Society dues could be reduced, for a large share of the dues of many societies goes to the support of their journals. Thus the average biologist would be able to afford a subscription to the combined journal and at the same time he would be able to buy microcard or microfilm copies of all the extended papers he wished to have.

I have tried to do a little guessing about probable costs. My guesses are probably inaccurate, but perhaps they are roughly approximate to the truth and will suggest the desirability of further study. I have based these estimates on an article in *Editor and Publisher* for 17 April 1954, entitled "50,000 circulation daily." At intervals this journal reports on income and expenditures of a typical but unidentified newspaper. The particular newspaper treated in this article last year had a circulation of about 50,000, an income of $2,657,468 and expenditures of $2,265,135. Two items of expenditure would not apply to a scientific journal —taxes and the cost of gathering material. Most material would not have to be solicited by a scientific journal. The cost of subscribing to the various news services, which is a major expense for newspapers, would be eliminated, and substituted for it would be the relatively minor cost of sending manuscripts through the reviewing process.

The newspaper referred to in this article issued 307 numbers during the year, containing 706,887 column inches of editorial matter averaging about 50 words/in., or about 35,344,000 words. This is more than 500 times as many would be published by the *American Journal of Botany* in a year's time if it printed only digests. It would be 50 times as much as would be printed in the combined scientific periodical which I have visualized if 10 journals the size of the *American Journal of Botany* went together to form it. Let us assume, to be very conservative, that it would cost twice as much per word to publish a journal with one-fiftieth the content of this daily newspaper as it does to publish the newspaper itself. This would mean roughly an annual expenditure of about $100,000 per year. Assuming a combined circulation of 30,000, this would mean a cost per subscriber of between $3 and $4 a year. If circulation were only half that, or 15,000, the cost would be between $6 and $8. When one considers the return from advertisements, and the fact that libraries can be charged more than individuals, the subscription price to the individual could probably be kept as low as $3 to $5 per year even if the circulation were not more than 15,000. For this, the individual would receive the equivalent of a subscription to 10 journals, which at present would cost him 10 times as much.

Although these figures may not be wholly accurate, I believe that the general order of magnitude is essentially correct and that biologists would be far better off financially under such a system of joint publication. It is probable, of course, that the quality of printing and especially of half tones would not be the equal of that in the best scientific journals at present. I believe, however, that they would be adequate for all but the most critical cases, and there would probably still be some journals that could handle papers requiring special treatment. It might not be impossible for the combined journal, at the author's expense, to provide illustrations on special paper by special methods; or illustrations could be segregated into a special section comparable to a rotogravure section.

What I have said does not take into consideration the probable economies that will be brought about when newer photoengraving and electronic techniques begin to supplant letter-press methods. These newer techniques may help materially to solve the cost problem. I do not think, however, that we will solve the problem of rapidity of publication until the various journals consolidate in such a way that copy-editing and manufacturing functions are in the hands of full-time skilled experts—and the nearer we can come to adopting the streamlined manufacturing methods of the average newspaper, the better off we will be.

But all of this looks into the future—I hope that, with the aid of the American Institute of Biological Sciences, it may not be the too distant future. In the meantime we have the immediate problem of presenting our material in such a way that it will be read. As the reprints and journals pour in, most of us glance through them. Many of those which are short and succinct we read at once. Others which are longer have good, informative summaries, and we read these. Still others have no summaries, or their summaries tell little or nothing about the findings and conclusions, merely indicating the problems attacked and the questions considered. These we set aside for a period of leisure that often never comes, and finally many of them get buried, unread, in the reprint file. Still another category, surprisingly common, includes the papers that are so obscurely and ambiguously written that one cannot just sit down and read them through, but one has to study and analyze them to find out what the author means to say. Some of the most prominent biologists are among the worst offenders in this respect. I am told that one prominent biologist goes off to the country over the week end and comes back on Monday with a new paper ready to be typed. The result is an obscurely and illogically written paper that can be understood only by the expenditure of much precious time by even the expert reader. I believe that the practice followed by some authors might well be adopted by all. This is to salt a manuscript away when it is finished and forget about it for a month or two, then pull it out when the details of expression have been forgotten and read it over.

One comes at it in this way in very much the same position as the person reading it for the first time. As a result the ambiguities, the omission of logical steps, the redundancies, are likely to be caught.

The most important points to consider, then, in preparing a paper so that it will be read are (i) to make it as brief as posible; (ii) not to be too hasty in sending it away, but to set it aside for a time before making the final revision; and (iii) to include an informative summary or abstract that succinctly outlines the major findings and conclusions. I believe that even a short paper should include such a summary; in fact, I believe that all editors should require summaries for all papers that exceed a page or two in length.

The late C. R. Stockard used to classify people into what he called "linears" and "laterals." Linears had as one of their traits a tendency to be conscious of the impression they were making when speaking or writing—they were conscious of listener or reader reaction. The laterals tended to be too much wrapped up in themselves and their ideas to think much about how others were reacting toward their speeches or writings. The plight of the scientific reader, and that includes all of us, would be much improved if more authors were more conscious of their readers when they put their thoughts on paper—logical sequence, clarity, and brevity would become more characteristic of our literary efforts if we had more ability to look at what we have written from the standpoint of the person reading the material for the first time.

Human nature being what it is, however, perhaps we will not reach the point where the needs of the reader are fully met until we devise some system for the publication of shortened forms of scientific articles supplemented by devices for making full length publications available to those who need them. Possibly the suggestions that I have offered, while too radical in the eyes of many, will start some individuals with more fertile imaginations and more technical knowledge than I possess to thinking and planning, and maybe we will in time achieve a world which will be at least as much a reader's as a writer's world.

Papers of Wilbur and Orville Wright

Nicholas J. Hoff

Department of Aeronautical Engineering and Applied Mechanics, Polytechnic Institute of Brooklyn, New York

FEW, if any, technical developments have brought about changes in transportation, commerce, international relations, and warfare to the same extent as the airplane. Since the greatest contribution to this development was made by Wilbur and Orville Wright, a detailed account of their lives, work, and thoughts is of immediate interest to every educated man. The story of the Wrights has been presented in two volumes under the title *The Papers of Wilbur and Orville Wright.** The particular attraction of this presentation is that the story is told by the Wrights and by their friends and business acquaintances in their own words; almost the entire book is a verbatim reproduction of correspondence and of diary entries in chronological order. Even though the papers are full of technical detail, the human story emerges from them in a dramatic manner.

In the late 1890's two young men became interested in the possibility of human flight. Fortunately, their business, a bicycle shop in Dayton, Ohio, provided them with a long slack season beginning in September and allowed them to devote a good deal of their time to developmental work on airplanes without undue financial loss. At the very outset, in 1899, they invented the device of warping the wings for control about the longitudinal axis of the airplane; this became the foundation of their later patent claims. In the spring of 1900, Wilbur Wright approached Octave Chanute, 35 years his senior, a successful civil engineer and businessman as well as author of the famous book on aviation, *Progress in Flying Machines.* His letter began with these words:

> For some years I have been afflicted with the belief that flight is possible to man. My disease has increased in severity and I feel that it will soon cost me an increased amount of money if not my life.

This was the beginning of a long friendship in the course of which many problems of aerodynamic lift and drag, wind-tunnel measurement, airplane stability, glider design, performance calculation, and the like, were discussed in a correspondence that takes up the greater portion of the first volume.

As is shown by the letters, the Wright brothers attacked the problem of flight in a systematic manner. They started by reading all the relevant literature, continued by constructing and flying model airplanes, and proceeded to gliding and soaring. At Kitty Hawk, North Carolina, chosen for its soft sands and steady winds after a careful survey of the reports of the U.S. Weather Bureau, they were disappointed in the performance of their first glider in 1900. To discover the reasons for the discrepancy between expected lifting power and that realized, they constructed, after their return to Dayton, a wind tunnel

* *The Papers of Wilbur and Orville Wright.* Including the Chanute-Wright letters. 2 vols. Marvin W. McFarland, Ed. McGraw-Hill, New York–London, 1953. 1278 pp. $25.

in the room behind their shop and measured aerodynamic quantities deviating considerably from accepted values. Their ingenuity and their manual dexterity are evident from the measuring instruments built during this period. Details of the wind tunnel, of the instrumentation, and of the aerodynamic coefficients obtained are given in the appendixes to the first volume; the material is illustrated by many excellent sketches and photographs.

The scientific approach brought success; from 1901 to 1903 the Wrights made many glides at Kitty Hawk and, on 17 December 1903, Orville Wright was able to send the following telegram to his father, Bishop Milton Wright:

> Success four flights Thursday morning all against twenty-one mile wind started from level with engine power alone average speed through air thirty-one miles longest 57 seconds inform press home Christmas

Since the Wrights have been attacked widely for their commercial attitude and for their efforts to make a monopoly out of aviation and to exploit the monopoly to their personal advantage, it is interesting to follow the development of their thought on this matter in the contemporaneous documents published in these volumes. In 1901 Wilbur Wright wrote:

> The labors of others have been of great benefit to us in obtaining an understanding of the subject and have been suggestive and stimulating. We would be pleased if our labors would be of similar benefit to others.

In a letter to Wilbur Wright, Octave Chanute said in 1901:

> The view which you take, that the time spent in aeronautical investigations is a dead financial loss, is eminently sagacious and wise.

But after their success in 1903 they tried to establish their rights to a financial exploitation of the results of their work. An explanation for this change in attitude is given in a letter to Albert F. Zahm written on 22 December 1905:

> When my brother and I began experimenting in 1900 it was purely for the pleasure of it. We did not expect to get back a cent of the money we spent. Consequently we agreed with each other that it should under no circumstances be permitted to infringe upon the time and money needed for our business.
>
> The results of the experiments were made public promptly for the encouragement of others. But after several seasons we found ourselves standing at a fork in the road. On the one hand we could continue playing with the problem of flying so long as youth and leisure would permit but carefully avoiding those features which would require continuous effort and the expenditure of considerable sums of money. On the other hand we believed that if we would take the risk of devoting our entire time and financial resources we could conquer the difficulties in the path to success before increasing years impaired our physical activity. We finally decided to make the attempt but as our financial future was at stake were compelled to regard it as a strict business proposition until such time as we had recouped ourselves.

Volume II begins with the year 1906. A few letters to Chanute still discuss technical problems, but most of the material between 1906 and the death of Wilbur in 1912 reflects the increasing business activities of the brothers. These include their fight for patents in the major countries of the world, their efforts to sell their invention in these countries, and their suits for damages against persons infringing upon their patents. The letters also give a lively account of Orville's serious accident in 1908, of the brothers' travels in Europe, of the enthusiasm generated by their flights, of entertainment by aero clubs, scientific societies, and representatives of governments, of their training of pilots in Wright planes all over the world, and of prizes won and world records established. In 1911 Orville found time for some more soaring at Kitty Hawk, and excerpts from his diary give a terse account of wind, weather, and length and duration of the flights. In 1912 Wilbur writes:

> It is much more pleasant to go to Kitty Hawk for experiments than to worry over lawsuits. We had hoped in 1906 to sell our invention to governments for enough money to satisfy our needs and then devote our time to science.

Bishop Wright's diary contains this entry for 2 May 1912:

> Wilbur began to have typhoid fever; first diagnosed, by Dr. D. B. Conklin, as probably malarial fever, and later as typhoidal fever.

On 10 May Wilbur wrote his will, which contains this dignified and moving tribute to his brother:

> The entire balance and residue of my estate remaining after the satisfaction of the foregoing bequests . . . I give, will, devise and bequeath to my brother Orville Wright of Dayton, Ohio, who has been associated with me in all the hopes and labors both of childhood and manhood, and who, I am sure, will use the property in very much the same manner as we would use it together in case we would both survive until old age.

On 30 May 1912, Bishop Wright entered this obituary in his diary:

> This morning, at 3:15, Wilbur passed away, aged 45 years, 1 month and 14 days. A short life, full of consequences. An unfailing intellect, imperturbable temper, great self-reliance and as great modesty, seeing the right clearly, pursuing it steadily, he lived and died. Many called—many telegrams (probably over a thousand).

Orville lived to be 77, but the drama of the Wright brothers and of human flight ended with Wilbur's death. Consequently, the period from 1912 to 1948 is covered in the book by only 122 pages. They contain some interesting information on the Wright companies and on airplane manufacture in the United States during World War I. The appendix of volume II gives a description of all the Wright airplanes and engines (with many diagrams) and a complete listing of the published writings of Wilbur and Orville Wright, of their patents, and of relevant court rec-

ords; to these is added a bibliography of publications on the Wrights.

No book, however well written by another, can convey the same feeling of intimacy with the characters of the story as personal letters do. The Wrights live, think, fight, and love in these two volumes. They emerge from the pages as persons of great intellect and character. In spite of their lack of a formal education in science and engineering, they were outstanding engineers and scientists. At the same time, their protracted negotiations in Europe showed them to be energetic and hard-headed businessmen. The playfulness of many of their letters to their younger sister Katharine reveals their love for their family, and the interest manifested by their family in all their affairs shows that this love was returned. Their interest in matters other than aeronautical can be seen from many remarks. On free days between fatiguing business talks, Wilbur used to visit the famous picture galleries of Paris and Berlin. Orville often described the beauty of the sunset at Kitty Hawk in his letters to Katharine, and the whole family had a great deal of fun from the witty stories of the hardships the brothers had to endure when camping out. A quotation from Orville's diary reads:

> At 11 o'clock last night I was awakened by a mouse crawling over my face. . . . I found on getting up that the little fellow had only come to tell me to put another piece of corn bread in the trap. He had disposed of the first piece.

The right of Wilbur and Orville Wright to claim the invention of the airplane has often been challenged in the United States as well as abroad. The Smithsonian Institution conceded this claim only in 1942; previously it considered its own former secretary, the outstanding scientist, Samuel P. Langley, as the inventor of human flight in a heavier-than-air machine. Some Frenchmen wanted to establish Clément Ader, and some Englishmen considered Hiram Maxim as the first designer of successful airplanes, and the claim of the Brazilian, Alberto Santos-Dumont, that he carried out the first documented flight persisted for a long time. Of course, these men, and many others before them, contributed a great deal to the development of the airplane. The Wrights often acknowledged their indebtedness to Lilienthal, Chanute, and Langley. But all success in human endeavor depends on the experiences of those who lived before. Lindbergh's achievement is not lessened by the earlier flight of Alcock and Brown, nor is the fame of Columbus endangered by Leif Eriksen. Today, more than 50 years after the flight at Kitty Hawk, there are few people, if any, who do not want to do homage to the memory of Wilbur and Orville Wright.

This review would be incomplete without a few words on the work of the editor, Marvin W. McFarland. It was his duty to select for publication the material that was of importance to the history of aviation; in his task he has succeeded fully. He has also written an excellent 17-page introduction, and with his associates, Fred S. Howard and Arthur G. Renstrom, prepared the appendixes and added the copious footnotes. These latter are of particular importance. They follow up, in a truly amazing manner, references in the text to many now defunct periodicals that were published in the United States and in the countries of Europe. They also contain historic and bibliographic data on all the persons mentioned in the letters and diaries, and thus make the two volumes an exciting history of the early days of aviation. The presentation of the material is greatly aided by the 236 beautiful reproductions of early photographs.

A great advance in science, though nothing could at first sight seem less poetical, inevitably results in a change both in the style and in the substance of poetry, as well as in the taste that judges it. A whole book might be written on the influence of Copernicus on poetic production, and another on poetry as modified by Darwin. In Memoriam, *for instance, though written before the* Origin of Species, *is full of the thoughts which were soon to be clarified by that work, and could never have been written had not the* Vestiges of Creation *appeared shortly before; while, though Milton still hankered after the Ptolemaic cosmogony,* Paradise Lost *is in part the work of Galileo and Kepler. It is hard—if we may leap to a later date—to imagine the loss the literature of Germany and the world would have sustained if Goethe had not been a student of science. Faust is informed throughout by the new scientific spirit, alike in its doubts and in its certainties; the philosopher is the physicist of the early nineteenth century, and Mephistopheles is the darker aspect of the same philosophy.*—E. E. Kellett, The Whirligig of Taste.

Scientific Editorial Problems

The 3rd annual Conference on Scientific Editorial Problems was held 29–30 Dec. 1954 at the Berkeley meeting of the AAAS. Marian Fineman, chief of the editorial branch, Dugway Proving Ground, Dugway, Utah, was chairman. The program was divided into four main topics: (i) preparation of technical manuals for complex instruments; (ii) effective technical writing; (iii) scientific journals; and (iv) military and industrial technical reports. We present seven articles in this section, each of which is based on a paper presented at one or another of the sessions.

The Care and Training of Authors

Robert C. Miller
California Academy of Sciences

James Thurber has drawn a number of cartoons centered in the theme of the permanent warfare between men and women. I might, for my purpose, have paraphrased this theme and entitled my article, "The permanent warfare between author and editor." I have myself been alternately on both sides in this battle, which I suppose classifies me as a mugwump. A mugwump has been defined as a bird that sits on a fence, with its mug on one side and its wump on the other.

I became assistant editor of a high school paper in Uniontown, Pa., in 1915, and have been serving almost continuously in some kind of editorial capacity ever since. I have also been an author for a slightly longer period, and have published a modest number of scientific and popular articles in my own and other people's journals. On the whole I should say that it is most satisfactory to an author to publish in a journal of which he is himself the editor. This not only insures appreciative acceptance and early publication, but affords a reasonable guarantee that one's manuscripts, when returned with galley proof, will not be annotated with snide remarks on spelling and punctuation, or have the best phrases deleted with a blue pencil.

I lately discussed the substance of these remarks with a graduate of a school of journalism, who expressed surprise and commented, "In journalism school we were taught to treat editors with great respect." If this be the case generally, I wish all scientific authors might take a course in journalism, for I know of nowhere else that such worthy ideals are inculcated. Most graduate students learn from their professors that editors are a necessary evil, to be borne with or if possible circumvented.

The essence of the matter is that editors are tough and that authors are touchy. Each of these attitudes is completely understandable.

An author puts his best efforts into a paper. It represents months or years of painstaking research, long hours of work in the library, and finally the throes of literary composition. He knows, or at least fondly imagines he knows, how to read, write, and spell. When he has finished his manuscript, he is proud of it. Naturally he thinks it is good, or he would not have taken the trouble to write it.

So he sends it off to a scientific journal. In due course, after having been read by one or more referees, it comes back with a few suggestions from the editor. The title should be changed, say from "Absence of extraneous elements in the flora of Turtle Mountain" to "Prevalence of indigenous elements in a selected Nearctic alpine flora." The introduction should be shortened to one paragraph, the review of literature omitted, the discussion limited to 500 words, reference made to Smith's 1951 bibliography and no literature cited except papers subsequent to that date, and a short summary written for *Biological Abstracts*.

The editor is of course merely trying to be helpful, but the gratitude of the author seldom finds expression in paeans of appreciation. The situation can be epitomized in the story of the two Boy Scouts who arrived late at their scout meeting, breathless and disheveled, just in time to answer the question, "Did you do your good deed today?"

"Yes," they replied, "we helped an old lady across the street."

The scoutmaster was inclined to regard this as a rather modest good deed, especially when it had to be bisected between the doers. "Why," he inquired, "did it take two of you to help one old lady across the street?"

With one accord they replied, "She didn't want to go."

The reluctance of authors to cooperate with editors who are trying to do good deeds on their behalf is little short of phenomenal. I once received two manuscripts from an author in the same mail. One I accepted and the other I returned with the suggestion that it could be more suitably published elsewhere. The author replied with considerable heat, "If you won't publish this one, send the other one back too." On another occasion I returned a manuscript with the unusual suggestion, not that it be abridged but that it be amplified to include additional material. I received no reply and did not pursue the matter further. Quite a long time afterward I wrote this author for some information that I personally needed in his special field. He replied, "If I give you this information—will you publish the paper I sent you five years ago?"

I have said that editors are tough, and they need to be so, because it is their business to maintain standards and to prevent the immense and increasingly complex business of scientific publication from falling beneath "the reign of Chaos and old Night." It is my present purpose to inquire just how tough they ought to be and how they can make it stick—in other words

(incidentally, when I am editing other people's copy, I always cross out the phrase *in other words* and everything that follows), what are the editor's prerogatives, by what sanctions can they be enforced, and conversely, where is the line to be drawn that will prevent the editor from becoming completely authoritarian? After all, authors have certain rights, however minor and inconsequential they may be.

The first thing to be said is that the editor must know his business. Speaking now in my capacity as an author, I will say that only once in my life have I won an argument with an editor. That was over the use of a possessive pronoun with a verbal noun, in such an expression as "I look forward to his coming." I won by stating with great finality, "The gerund always takes a possessive." He subsided because he had never heard of a gerund and wanted time out to look it up and see whether such a thing really existed or whether I had invented it on the spur of the moment.

Actually if he had been an editor worth his salt, he would have replied without turning a hair, "It may be true in general that the gerund takes a possessive, but it is not true in the case of this magazine."

Seriously, an editor has to have an immense and ready knowledge of grammar, spelling, punctuation, capitalization, permissible variant spellings, foreign words and phrases, the International Rules of Zoological and of Botanical Nomenclature and wherein they differ, and the special vocabularies of a dozen different sciences. He must know why it is often correct to capitalize the specific names of plants, but never the specific names of animals. He must know that new species of animals can be described in any language from English to Sanskrit, but that new plants must be described in Latin, and he should be able to read and edit the Latin description (we shall not insist that he be able to read and edit Sanskrit, although that would help). He should know that the direction that may briefly if inelegantly be described as from back to belly is dorsoventral in zoology and dorsiventral in botany; also that *dorsi-* is correct in certain anatomical terms, as dorsispinal. He must also keep abreast of the times, and be aware that language is a living thing and that correct usage changes from generation to generation. There is no use in trying to convince an author of the error of his ways by citing the 1910 edition of the *Century Dictionary*, or the battered copy of Woolleys' ***Handbook of English Composition*** the editor used in college. Recent editions of Woolley even cite cases in which the gerund does *not* take the possessive.

Most important of all, the editor must have a nose for mistakes, whether of expression or of fact, which corresponds to a good reporter's nose for news. His mechanism for spotting errors or possible errors must be as sensitive as a Geiger counter.

Now that I have described in glowing detail this superman, let us consider just how far he is entitled to go in exercising his extraordinary talents. The general procedure is something like this:

If the editor has any major changes to suggest, he returns the manuscript to the author, offering his suggestions for revision. These suggestions the author may accept or reject, although it must be acknowledged that he is under a fair amount of pressure to make the changes if he wants to get his paper published. His alternative, of course, is to withdraw the paper and submit it elsewhere. If the changes are of a minor nature, the editor will probably make them on his own responsibility, and the author first discovers them when he gets the galley proof. He is quite likely in a moment of pique to change everything back to the way he wrote it in the first place. He returns the proof to the editor, who erases the author's changes, and sends the galleys to be paged up. In due course the author receives a page proof, and by this time being madder than a wet hen, he reverses all the editorial changes, and does it in ink. However, the corrections in ink do not have the finality he thinks, because the editor has kept a duplicate page proof, which is the one he will, after due deliberation, send to the printer.

It is this final page proof that involves the editor in deep searching of soul. How far shall he compromise with the author, and how far shall he insist on the alterations which he thinks are desirable for the improvement of the paper or necessary for the maintenance of standards of scientific publication? I do not know the answer to this problem; I can only tell you the formula I have arrived at and how I reached it.

When I was a young editor, slightly past the high school phase but still not quite dry behind the ears, I was a great stickler for consistency. Every paper had to be set up in the same way; citations of literature had to be made in parentheses, by author, year, and page, referring to a terminal bibliography, which also had to be set up in a standard manner.

I was unhappily but usefully shaken out of this fool's paradise by a two-year stint in China as editor of the *Lingnan Science Journal*. In those two years I got out 2600 pages of scientific publications, written in French, German, and English and interlarded with Chinese. My principal German contributor corrected his proofs in German script, which is one of the best ways I know to get even with an editor. My Chinese compositor did not read English, but set type by picking out of the font the letters that looked like the copy that was given him; actually he did phenomenally well, but inevitably he mixed up letters like lowercase *b* and *d*; and for the first time I learned the literal meaning of minding one's *p*'s and *q*'s.

After two years of this I found that I had not entirely lost my editorial ideals, but I had modified them. The immovable object had yielded to the irresistible force. I arrived at the conclusion that consistency is an unattainable ideal. I do not mean that it should not be striven for; I mean only that it cannot be attained.

I still like the method of citation of literature I have described, a method that I learned at the University of California more than 30 years ago. I still

follow it in my own publications. I follow it as far as possible in my editorial work. But when I was editor of the University of Washington *Publications in Biology*, an entomologist turned in a paper that did not conform to this formula. I called this to his attention and suggested that he be guided by precedent. This was the wrong thing to say, because he immediately produced innumerable examples to prove that entomologists cite their literature immediately at the beginning of each discussion of a species, and that they may or may not include a terminal bibliography.

I was convinced, and agreed that the entomologists could follow their professional idiosyncrasies. This practice was allowed, and we follow it also in the *Proceedings* of the California Academy of Sciences. I think any journal that publishes papers in more than one field has to allow this kind of latitude, however deeply it may grieve the editorial spirit.

In matters which, in the final proof, still remain moot between editor and author, I proceed as follows: I correct positive and provable errors. I will not permit words to be hyphenated in the wrong place, or a singular noun to be followed by a plural verb, or a plural noun like *data* to be used with a singular verb. I still hold out for the correct cases for pronouns, although I am weakening, and may in another 10 years yield to such an expression as "It was him." But I have given up worrying about *shall* and *will*, split infinitives, and terminal prepositions. I make my suggestions, and the author can take them or leave them. After all, it is his grammar and not mine that the reader will judge, if indeed the reader pays any attention.

To those editors who feel that I am abandoning the real line of defense and retiring to an inner citadel that must ultimately fall, I shall offer this suggestion: a great deal of the difficulty between author and editor could be resolved, and the work of each made easier, if the editor would tell the author what he wants.

Shifting gears now from the third to the second person—if you are going to maintain specific standards, tell the author what your standards are. State under your masthead that manuscripts must be typewritten, double-spaced, on one side of the page, and should conform to the University of Chicago *Manual of Style* or some other—your own, if you care to write one. As a matter of fact, most things that most editors insist on can be spelled out on one mimeographed page, which can be supplied to a prospective author on request, or sent to him with the return of an unsatisfactory manuscript, with the suggestion that he comply. This avoids a great deal of argument, debate and needless correspondence, and puts the editor in a position that can be defended by logic instead of *force majeuré*.

In conclusion, I should like to return to the terminal preposition. What should be the last word on this has been said by Winston Churchill. When an editor recast a sentence of his that ended with a preposition, Churchill changed it back to the original form and wrote in the margin, "This is the kind of nonsense up with which I will not put."

Most of the problems that arise between editor and author can be alleviated by a sense of humor and resolved to their mutual satisfaction by the use of plain common sense.

Security and the Editor

Nash Candelaria*

North American Aviation, Downey, California

Security, as it affects editors, is more than a conscientious locking of classified material in safes after the working day is through, or keeping a closed mouth on classified work. Both research people and those who enforce security seek, in addition to the safeguarding of data from unauthorized persons, the widespread transmission of scientific information as rapidly as possible. One of the big problems of security is that of proper balance between rapid and widespread transmission of information and iron-clad security. It is in this area that the industrial technical editor working on classified projects can contribute. Because research publications pass through his hands, the editor is in a position to maintain as a goal the classification of reports at a minimum consistent with safety.

To be more specific about the editor's contribution to security let us consider a typical research organization doing work under contract to the Atomic Energy Commission. Every research report which the research department issues goes to the publications unit for final preparation. Each report is assigned to a particular technical editor. He is responsible for editing, necessary rewriting, and some of what might be called report production, that is, the detailed work necessary for preparation of correct reproducible copy for a print shop. In addition to his editing duties, the editor may be called upon from time to time to write special material for various purposes.

From the day a technical editor is hired he is submerged in security. For example, let us take a hypothetical case, that of technical editor L. Drake. Like many editors in research organizations, Drake had a degree in a physical science with some laboratory experience, as well as training and experience in a writing and editorial capacity. He was trained to understand technical material; he had a strong interest in editorial problems.

Immediately after he was hired Drake's name was submitted to a preliminary security clearance. Within a few days he was given a security lecture. He was allowed to work only in a nonrestricted area while he was undergoing a complete security investigation. During the time of the complete investigation he was not allowed to work on classified material or to enter the restricted area. Drake was certain that his loy-

* Present address: Beckman Instruments, Inc., Fullerton, California.

alty, character, habits, and associations were above reproach, and he hoped that his investigation was being expedited so he would soon be able to enter the security area. He heard from neighbors, friends, and previous employers that a government investigator had interviewed them. Then for a time he heard nothing. One day the senior editor came in. He told Drake that he had been given a clearance and that he could enter the restricted area.

Now Drake's contact with security was even more constant. He was stopped at a guard station and the guard entered his name on a roll and studied his face. From now on Drake would be admitted to the area on recognition by a guard. Later that day Drake was given a copy of the department security guide. So far he had been immersed in elements of security that affected every other individual in the department, whether or not they were editors. It was when he went to work that Drake's problems became unique.

Drake's first assignment was a report on chemical research written by a member of the chemistry group. This was the type of work Drake could do best. Read the text, understand the chemistry, delete commas, correct misspelled words, rewrite the few sections that seemed to need revision. This was easy. Then to the art work. Check the spelling of words, indicate directions for the illustrator, order necessary photographs, then prepare a dummy layout of the finished report. Nothing to it. Nothing to it, that is, until senior editor Jones stopped by to see how Drake was doing.

Jones checked through the deleted commas, corrected words, and rewritten sentences. He suggested that Drake check these out with the author of the paper. "And be tactful," Jones said. "You know authors." Drake smiled. He had met a few.

Then Jones checked through the art work. Several of the photographs had security classifications marked on them. Several of the photographs did not. Jones picked up the photographs without security classification marks. "What are the classifications of these?" he asked. Drake shook his head. He did not know. Then Jones picked up the photographs with security classification marks on them and scrutinized them. Two of them were photographs that had been in their files for some time. The subjects photographed were no longer classified.

"How do I know these things?" Drake asked.

Question everything, he was told. Check your security guide. Check also appropriate laws, regulations, and directives. The security guide is a summary, not a substitute. Then ask the author questions. He is responsible for assigning a classification to newly prepared material. If he does not know, check with the department classification officer. He is the final authority within the department. For old material that may have had its classification downgraded, check with the declassification officer. This declassification is a continuing process. Over a period of time you will become familiar with the projects, the art work, the photographs. You will have a better over-all grasp of the work of the department through handling its research reports than almost anyone. You will get to know what is and what is not classified. By checking you can save time and trouble later.

Being conscientious, Drake heeded Jones' words. He looked through some of the report material that had to be drawn by an illustrator. None of the drawings had classification markings on them. They, too, would have to be checked. He gathered the material that had been rewritten and the material on which there was some question of security classification and went to the author.

Drake discussed the rewrite suggestions with the author. The author, like most authors, liked his way better, but he was agreeable. Yes. There were certain changes which were improvements. There were others he would prefer his own way. Drake then asked about security classifications for the illustrations. "I'm not sure," the author said. "It's a secret report. Make them all secret. That should certainly give them adequate protection." Drake jotted down *secret* on a notebook pad. Then he went back to work. He did not want to use too much of the scientist's valuable time.

The report was processed and the copy prepared for reproduction. The material was passed around for final approval. No one questioned the wording, the punctuation, or the classification of illustrations. They relied on Drake's judgment. The report went to the print shop, then two weeks later it was returned. There it was, in black and white, for everybody to see.

The report was mailed according to official distribution lists for classified materials. Many of the illustrations indicated as secret were overclassified. That is, some of them that had at one time been classified secret were no longer secret. Some of the newly drawn ones were not secret. Several research people needed copies of the photographs and drawings for a special unclassified booklet to be issued. Since the illustrations were marked secret, they could not go into an unclassified report.

Time was spent checking and rechecking the proper classifications of these drawings. Then, to be on the safe side, the illustrations were submitted for declassification. This involved filling out, in quadruplicate, a declassification form. One copy of the form, together with one copy of the material to be declassified, was sent to a responsible reviewer, a technical man in the field who passed judgment relative to the declassification. Another copy of the form and a copy of the material were sent to the Chief, Patent Branch, AEC, Washington, D.C. One copy of the form and eight copies of the material were sent to the Chief, Declassification Branch, AEC, Oak Ridge, Tenn. The responsible reviewer and the patent reviewer made appropriate comments and then forwarded the material to the Declassification Branch. The Declassification Branch passed on the declassification and sent back one officially declassified copy. All recipients of the material were then notified. This time and effort could have been saved by the editor or author. How was the release of the unclassified report affected? The report came out much later than desired. In this particular

case it did not matter. In another case it might have been detrimental to some important program. You can be certain that Drake handled his next assignment correctly. He checked with the author, the classification and declassification officers, and he used better judgment.

An editor's job is not always concerned with classified technical reports. Because of his literary training and/or interests, he is often called upon for assistance on special writing jobs. For example, a department security guide was recently prepared for distribution to all persons dealing with classified material. Pertinent information was extracted from official laws, regulations, and directives. A technical editor did much of the writing work. After finishing the job, the editor sought the help of an artist to prepare small posters for a program of security consciousness. The guide and the posters were submitted to the appropriate AEC offices for approval before distribution.

Thus it is clear that security, as it affects editors, is more than a conscientious locking of classified material in safes after the working day is through, more than keeping a closed mouth on classified work. It involves a growing awareness of the work undergone in the research organization. It requires recognition of what is or is not classifiable. It requires decision and judgment. It requires the necessary tact and thoughtfulness in dealing with others, especially when you believe the author does not have the correct security information. Above all it requires a constant awareness of a goal: the classification of reports at a minimum consistent with safety so that data can be properly safeguarded and yet rapidly distributed to those who need it—for only under such conditions can American science maintain its technological superiority.

Is There Accepted Scientific Jargon?

D. T. McAllister

U.S. Naval Ordnance Test Station, China Lake, California

In the lobby of the Michelson Laboratory at the Naval Ordnance Test Station, China Lake, there is a full-size dummy of the Tiny Tim, a large aircraft rocket used in World War II. The Tiny Tim is now definitely in the public domain—if I may extend the use of publishers' jargon to the field of military security. Any casual visitor could look through the cutaway wall of the rocket motor and examine the propulsion system.

If you looked carefully you could see that the propellant charge consists of four cruciform grains of double-base powder with the peripheral surfaces of the arms partially inhibited in a helical pattern. Of course some, not knowing what to look for, might not see all that I have just described, although I would be willing to bet that even then you would have a much better idea of what makes the rocket go than you now have from the language of my description.

Some of you are no doubt puzzled and a little frustrated, but you are no more frustrated than I was when I first started editing reports on rocket development. At that time such a description would have bothered me a great deal. It probably sounds to you, as it once sounded to me, very much like standard English at the college level, with a liberal sprinkling of polysyllabic words, all of them to be found in a good desk-size dictionary. And yet somehow it fails to communicate clear information to a literate person who is not at least something of an expert in rocketry. Is it, perhaps, a simple example of *scientific jargon*? An example that does not contain a single coined word?

It is special language of some sort. Yet it is definitely not jargon in the sense of "chatter or twitter, as of beast or bird"—a meaning now rated obsolete, but a meaning still recognizable as a logical one for a word probably derived from the same source as *gargle*. Nor is it jargon in the current meaning of man-made "gibberish" or even "confused and unintelligible language" in the sense of a "barbarous or outlandish dialect."

Indeed, even after close examination most of the words appear to be burdened with little more than their usual meanings: for example, *charge, cruciform, peripheral, arms, helical,* and *pattern*. The rest of them appear to be, in this context, conveyors of precise special meanings beyond your present power to interpret.

Do the words *propellant, grains, double-base, powder,* and *inhibited* then represent jargon in the sense of "the technical, esoteric, or secret vocabulary of a science, art, trade, sect, profession, or other group?" Or are they merely the verbal "circumlocutions and long, high-sounding words" of the final definition of *jargon* in *Webster's New International Dictionary*?

When I first met them 10 years ago, these words were already established elements of the local technical vocabulary, and everybody used them. In time I learned, by asking questions of our experts and reading the reports of others, that they were definitely esoteric in their special applications. Once initiated, I too could understand their inner meanings.

I had to learn that a solid *propellant* of the type used in the Tiny Tim is a plastic substance, usually dark gray in color; that it is indeed the "fuel plus oxidizing agent used by a rocket engine" (as explained in the recent printings of *Webster's New Collegiate Dictionary*), but at the same time is definitely not "a explosive for propelling projectiles." Most certainly it *deflagrates*, or *burns* "with sudden and sparkling combustion," although we at the Naval Ordnance Test Station usually do not encourage the sparkling. But it never detonates, or explodes "with sudden violence" —or at least it is not supposed to; for when it does, it causes trouble at the wrong end of the trajectory.

And I also had to learn that *grains* of solid propellant are often several inches in diameter and several feet long; that *double-base* means that the formula includes both nitrocellulose and nitroglycerin; that the *powder,* before it is processed into *grains,* bears no

physical resemblance to the powder you put on your toothbrush, the powder you dust on your face, or the gunpowder in your shotgun shells, but that it most closely resembles rolled-up strips of battleship linoleum; and finally, that although the term *inhibited* can claim as an ancestor the common transitive verb *to inhibit* in the sense of "to hold in check" or "to restrain," in my example it means "covered with a slow-burning substance so as to retard combustion locally."

Now that I have initiated you into the rocket fraternity, you will at once agree that my description of the source of motive power in the Tiny Tim is not, in fact, "language full of circumlocutions and long, high-sounding words." Rather, it illustrates, in an elementary sort of way, the legitimate use of specialized vocabulary by one expert in a narrow branch of science to communicate exact information to other experts in the same branch with genuine economy of expression. In other words, it is an example of acceptable scientific jargon.

To test the economy factor, just try rewriting the example in what you consider truly standard English, even at the college level—without loss of meaning, that is. You will find your task nearly as difficult as trying to express the full meaning of the apparently simple equation, $e = mc^2$, in nonmathematical language.

Thus we are forced to conclude (i) that there *is* accepted scientific jargon and (ii) that without such jargon specialists would find communication at least as awkward and time-consuming as doing multiplication with Roman numerals. But finding an affirmative answer to the question, "Is there accepted scientific jargon?" leads only naturally to several more troublesome questions. These further questions are strongly implied in the letter to the chairman of the Conference on Scientific Editorial Problems, in which this topic was first suggested.

"Much of the editing of scientific papers is done by people not expert in the field," says our correspondent. He then points out that editors find real difficulty in differentiating between "accepted" and "nonaccepted" jargon. As one example, he mentions the frustrated editor of a medical textbook who, not yet having reached expert status, altered the expression "diabetes insipidus"—and thereby profoundly disturbed his author. The editor evidently failed to recognize "a diabetes insipidus preparation" as accepted scientific jargon for "a dog that had been artificially made into a diabetic."

As another example, the letter writer mentions the English scholar turned technical editor—in desperation, perhaps to keep himself housed, clothed, and fed these days. This sensitive soul, who in his previous incarnation learned to revere Joseph Addison, the master essayist, balks when he is faced in his present incarnation with the term *Addisonian* meaning "a person with Addison's disease."

You will now recognize the following three questions as those to which we must find answers. (i) Is there a simple touchstone for determining acceptability of scientific jargon? If not, how does the inexperienced editor—or any editor, for that matter—find out whether or not the jargon staring him in the face from the manuscript page has actually been accepted in the subject field, and hence may be presumed to convey real meaning to the specialist reader without further definition? (ii) When is scientific jargon appropriate and desirable, and when should it be avoided? (iii) How does an editor overcome his mulish tendency to balk at new words, and particularly at old words with new specialized meanings?

Let me suggest a few answers. But let me begin with the last question and work backward, for an editor's first problem is to break down his own prejudices.

A truly competent editor, in my judgment, does his thinking in terms of the author and the readers of the book—not in terms of himself. His primary interest in the job, aside from his pay check, is to make sure that the author's intended meaning is conveyed accurately, efficiently, and effectively to all the readers for whom the book is being written.

He must learn not to edit into his author's manuscript the idiosyncrasies of his own personal style. He must, indeed, become a sort of Jack-of-all-trades in matters of style. Perhaps that is why an outstanding writer rarely makes an outstanding editor.

Similarly, he must train himself to think about words the way a lexicographer does. He must accept the fact that most words have several meanings, or exact shades of meaning, and that these are constantly changing. He must learn to identify these meanings with precision, and then to sort them into well-defined compartments by subject specialties. In the process, he will come to recognize his own preferences for what they are: the meanings that belong in the subject compartments where he has spent most of his time to date. Thus, he will learn to become objective in evaluating terminology, as well as successful in keeping his blood pressure down when he stumbles against a new meaning that at first repels him.

A recent experience of my own with the word *feedback* may help clarify the point. As one of a group of supervisors, I attended a seminar on management. Very early in the course, we began to discuss group dynamics. I was startled, and even a little offended, to hear my old friend *feedback* being tossed around rather carelessly.

I had first met *feedback* when I was a radio ham in the early 1920's. The regenerative circuit was new then. *Feedback* meant just what *Webster's* still says it means, "the return of a fraction of the output of an electric oscillation to the input to which the fraction is added at the proper phase," with the understood purpose of increasing the amplification or sustaining the oscillation.

But here were the management experts using the same word with some such meaning as "distorted information returned to the originator and used by him to check the effectiveness of his attempt to communicate undistorted information in the first place." Ever since, I have watched for different uses of the term.

The following are samplings of what I have found. The *Electronics Dictionary* (*1*) issued in 1945 and the *Dictionary of Guided Missile Terms* (*2*) issued in 1949 both recognize an expansion of the earlier meaning to include acoustic as well as electric output, but without any indication of a "control" or "checking" function. Stuart Chase in his recent book, *Power of Words* (*3*), comes out flatly and says, "A short, handy definition of a feedback is that it answers the question: 'How am I doing?' "; also, "Feedback is the control of a system by reinserting into the system the results of its performance." And even more interesting is this sentence from the October 1954 issue of *Audio* (*4*), which occurs in a discussion of a new system of tone generation for electronic organs: "This 'feedback' tends to reduce stability since it does not give complete control of the high-frequency master oscillator." But in the last instance, please notice, *feedback* is in quotation marks.

As a consequence, I can now distinguish between the precise use of the word by the electronics engineers and the more colorful special application to which it is put by industrial psychologists. And I have become a better editor in the process.

Now the second question should be an easy one for an editor on the staff of a technical journal. He would answer, I suppose, that scientific jargon is appropriate when it has become accepted in the subject field, and is desirable whenever its use saves space. Or is it really that simple?

But for those of us who edit for a mixed audience of specialists and administrators, the answer is not so easy. Some say, "Ignore the administrator; he won't understand anyway." And I ask, "Dare we ignore the holder of the purse strings?" Others, lacking experience, suggest avoiding scientific jargon altogether or defining each special term on first occurrence. Such an approach is just not feasible, however. Remember the propulsion system of the Tiny Tim.

The only answer is to make sure that the information of interest to each audience is presented in the terminology that each will understand most readily. On the one hand, this means liberal use of accepted scientific jargon in statements of technical problems, findings, conclusions, and recommendations for the specialist. On the other hand, it means furnishing the administrator with the general information *he* needs in *his* own terminology, together with a brief interpretation of at least the over-all problems, conclusions, and recommendations—but without the use of scientific jargon unless it is explained in standard English or is essentially self-explanatory.

Sometimes the end product takes the form of separate publications for specialist and administrator. More often, the best solution seems to be a special summary, written with the administrator or general reader in mind and bound into the basic technical publication. Usually, however, we do the best we can by simply trying to word the introductory and terminal sections of a single publication so that they are intelligible to both audiences.

The remaining, and probably most difficult, of the three questions I shall leave mostly to you. I have found no one touchstone for scientific jargon. We collect dictionaries by the dozen at the Naval Ordnance Test Station, for our subject matter threatens to include the entire range of the physical sciences. But no competent editor of ours would dare conclude that a technical expression had not yet been accepted just because he could not find it in a dictionary. Our fields of interest are changing too rapidly for even the special glossaries to keep pace with the changes in thinking.

The result is that we still do as I did in my early days of editing. We turn to a recognized authority in each field, when we can find one easily. We also consult the literature when we have time—especially the latest textbooks and the reputable journals. What we find there, we assume is, or will soon become, generally accepted terminology. Do their editors not have a much better clearinghouse than we? Do they not steep in their individual subjects, whereas we merely steam momentarily in one subject after another?

Otherwise, I must confess, we usually go along with even our junior authors, trying to insist that they define on first occurrence those terms that are noticeably new even in our local vocabulary.

I believe that there is indeed such a thing as accepted scientific jargon; that acceptance is determined mostly by usage; that such jargon is appropriate and desirable whenever it serves as a vehicle for accurate and efficient communication between experts, but not otherwise, and that it behooves an editor to learn to work skillfully and at ease among these words of special meaning.

References

1. N. M. Cooke and J. Markus, *Electronics Dictionary* (McGraw-Hill, New York, 1945).
2. Committee on Guided Missiles of the Research and Development Board of the National Military Establishment, *Dictionary of Guided Missile Terms* (Public Affairs Press, Washington, D.C., 1949).
3. S. Chase, *Power of Words* (Harcourt, Brace, New York, 1954), pp. 44–46.
4. R. A. Dorf, "Audio Patents," *Audio* **38**, No. 10, 2 (1954).

Use of the Technical Report in Military Planning

Donald H. Hale

Army Chemical Corps,
Research and Engineering Command,
Army Chemical Center, Maryland

In discussing the use of the technical report in military planning* I propose to interpret the term *military planning* as planning for tactical or strategic operations. Planning for military organization, equipment, logistics, and training is certainly a real part of all planning for tactical and strategic operations. The scientist and engineer record their experiences and to a degree their opinions in the technical report.

* Cleared for publication by the Office of Public Information of the Department of Defense on 2 Dec. 1954.

The military man plans changes in organization, equipment, logistics, and training in order to take advantage of new developments. After this is done he changes the military doctrine in order to take full advantage of the new organization and equipment. In times of emergency the strictly military planning often follows very closely after the development work has been completed and the technical report has been written.

Vannevar Bush has stated in *Modern Arms and Free Men* that by 1918 the principal devices of modern war—automatic guns, self-propelled vehicles, tanks, aircraft, submarines, radio, poison gas—had been tried out in practice. In addition, mass production had appeared, and the petroleum, automobile, chemical, and communications industries had approached maturity. Bush also states that the world made almost no studies of these devices and methods in the period between the two wars.

If we grant that Bush's analysis of the situation is essentially correct, and I think we must, then we must decide that something was wrong in our country and that we could have been much better prepared in December 1941 than we were. In these troubled times it is not likely that we will again be allowed the time to plan the application of advances in science and engineering that have been made in a period between wars after the fighting has broken out.

One might say, somewhat dogmatically, that the failure of the country to plan to take military advantage of technologic advances during the period between World War I and World War II was the result of the failure of the technical men to sell their ideas to the military, to the Congress, and to the American people generally. Actually, as we all know, this is a much-too-simplified generalization. During most of this period the American people were not interested in such things, and a regiment of Daniel Websters could not have roused them from their apathy. Today the situation is quite different. But even after December 1941 we still had considerable difficulty in utilizing the products of the laboratory and civilian engineering in our war effort. All sorts of reasons were given for our difficulties. The civilian technical worker was inclined to say that the military men had so little technical training and so little knowledge of technical matters that it was almost impossible to get them to accept any new idea. The military man, on the other hand, was inclined to view all civilian technicians as impractical dreamers and applied the term *long hairs* to the lot. I am happy to report that these days one almost never hears this term of derision used by military men. It appears that we do make progress.

Actually the military man's position, when he is required to decide upon the adoption of a new device, a new technique, or a new weapons system, is a very difficult one. Almost always he must decide to abandon a system that has worked successfully in the past. His decision to adopt a new item or a new system almost always means that much costly material must be junked before it is worn out or used up. His decision may well have real impact upon the production of the country and this is a serious problem in the middle of a war; further, and most serious of all, when the military man makes the decision to adopt new materiel or procedures, he is to a point gambling with the lives of many young men. He must be absolutely certain that the adoption of the new device will not, at the very least, cause the nation to expend more lives in winning the war at hand or the war that may be fought later.

All this tends to make the military man cautious and conservative, as who among us would not be if he were forced to make decisions of such magnitude in such a climate. To express it in the language of the salesman, the military man is a tough prospect. And yet the technical man must often, if he is to be true to his country and to himself, use every means at his disposal to sell a new idea to the military man. All of us who have any part in military research and engineering have a responsibility to keep senior military men fully informed of the progress of our work and constantly to point out the possible applications to military operations. We do this through the medium of the technical report. The report may be written or oral, and it may be delivered formally in the briefing room or informally in a club car, over dinner, or even at a cocktail party. We are fortunate today because the technical men of the country are fully aware that the responsibility for planning for the use of the latest technologic advances is no longer to be borne by the military alone. The scientist and engineer are ready to assume a large share of responsibility.

The scientist or engineer who really believes in himself and his ideas and in his work must use every avenue to sell the product of his work. If you are inclined to scoff at the informal and indirect approach, I call your attention to the Shepley-Blair report and to the manner in which the possible use of atomic power for military purposes was brought to the attention of the President when the last war broke out.

The technical report is always important and if the material reported upon has a bearing upon our national defense it is doubly important. The report must certainly follow some format or other. Many are used and as long as the material is presented in some logical manner I do not think that the format is very important; but the language used is of the utmost importance. The reporter is not writing entirely for the reader who is skilled in some technical field and who understands the language of the field. The governmental research and development structure has become so complex that reports will often be read by men who are not experts in any technical field. Yet these nontechnical readers must often make far-reaching decisions on the basis of knowledge they glean from the report. We must make every attempt to minimize the use of any vernacular that is peculiar to us or our group and to the workers in our field. I find the young doctor of philosophy particularly clumsy at writing his report in good English, English in which the use of simple words and phrases predominates.

Possibly the young man feels that he will be a disgrace to his university and to his teachers if he fails to demonstrate clearly that he is capable of using the technical terms that are peculiar to his craft.

The report must arrive at some conclusion. Administrators of scientific work and other planners must constantly make decisions concerning technical work that they cannot follow closely if for no other reason than that they do not have the time. These people are most interested in what the man in the laboratory thinks of his work, and the technical worker should sum up his ideas in his conclusions and sometimes in his recommendations. Yet many technical men seem absolutely incapable of coming to any conclusion concerning their work. Perhaps they write indefinite conclusions and fail to make recommendations on the grounds that this sort of action will relieve them of criticism in the future. But the conclusion is the meat of the report for many readers and it is particularly important for the busy administrator and for the reader who is not an expert in the field reported upon.

Without expending much effort I found the following conclusion in a report prepared in one of the Department of Defense installations. This report incidentally was one that certainly will be read by military officers who must plan for operations. The conclusion read: "The closed houses and slit trenches afforded some protection from the effects of the cloud."

Perhaps the technical man who is skilled in the particular field reported upon here can read this report and make up his mind whether the protection afforded is small or considerable. But what of the nontechnical reader? He is probably lost. It would have been much better if the writer had stated quantitatively what degree of protection was provided. He might be excused for hedging a bit and qualifying his statement by saying that his results were for some reason not too good and that his estimate might well be in error by such and such a percentage. But even so, he would be giving the nontechnical reader a much more precise idea of what he actually observed and believed than is given by the indefinite statement as written.

That the technical report is often used as the basis for military planning can hardly be questioned. The technical report is also often used by the administrator, who, unfortunately, is almost never highly skilled in the field reported upon, as a basis for decisions concerning the continuing effort to be placed upon that work. Two examples of how the technical report may be used directly by military men might be interesting.

In World War I the German chemists led by Haber conceived the idea that the stalemate on the Western Front might be broken by the introduction of gas warfare. They reported this to the German General Staff and convinced the chief that gas should be used. The technical problems involved were solved brilliantly by Haber, but it is clear that the military men were inadequately informed of the potentialities present. Hindsight tells us that if proper preparations had been made to take advantage of the gas attack, the impact on the Allies might well have been overwhelming. We must admit that the failure involved must be charged to the technical people. They had failed in their reporting.

However, the scientist does not always, by any means, fail to convince the military man. During World War II our troops ran into difficulties in the Pacific theater where the terrain was such that the Japanese could fortify caves. A team of military-civilian scientists was set up to study the problem. Their report indicated that flamethrowers of various types would be of real assistance, provided that special small troop organizations were set up to handle the weapons and that special types of training were given. Within a short period of time specially organized, equipped, and trained troop units made their appearance in the Pacific. It is commonly known today that these organizations were of real help in many fights. This was a case in which the technical men did an excellent job of reporting their work and made a real contribution to the war effort.

I wish to cite one other example of the difficulties experienced by readers when language is used in a clumsy fashion. This example also comes from a report written in one of the Department of Defense laboratories, a report that discusses some of the problems associated with the clean-up of areas and materiel after an atomic explosion. The report was written primarily for military staff officers who must use it to write military doctrine and training manuals for the use of troop commanders. It contains this sentence: "Dosage varies directly with the amount of contamination and with the length of exposure during decontamination." This sentence does not convey a precise idea; and the ideas conveyed do not seem to be technically correct. I have no thought of going into a technical discussion of the mechanisms by which penetrating radiation damages the human body, but I believe that the writer was trying to say that the radiation dose to which the worker might be exposed would depend upon the amount *and the type* of radioactive material in the vicinity and also upon the length of time during which he exposed his body to the radiation. I am quite certain that the reader of this report would gain incorrect concepts unless he had considerable knowledge of the physical and biological laws involved. The nontechnical reader might draw incorrect conclusions from such a statement and set dangerous procedures in action.

There is nothing to be gained by presenting more examples and further belaboring the point that clumsy use of the language can make a report useless or even dangerous. The technical editor must always fight against the poor use of the language. This may well be his most weighty cross, and he must bear it until he succeeds in training and convincing the writers of the reports that he edits. I am not convinced that writers and editors generally remember that the men who must study and analyze reports and make decisions based upon reports are not in general experts in the field of work being reported.

Clarity in Geological Writing*

K. A. Townley

Bureau of Mineral Resources,
Geology and Geophysics, Canberra, Australia

The natural scientist—the biologist, the geologist, and the physiologist—is faced with a considerably more difficult problem in communicating his ideas than is the physical scientist. The mathematician and the physicist, and to some extent the chemist, can legitimately use shorthand—formulas—to express their meaning; but the natural scientist is thrown back on his powers of description to express *his* ideas. And, paradoxical as it may seem, I think that much of the involved writing and unnecessary jargon that deface manuscripts in the natural sciences owes its origin to the subconscious quest for a formula, for a shorthand in which to relate one's observations and deductions simply.

But the English language is not susceptible of such treatment. Such attempts are bound to fail; and the failure is bound to induce in the reader a sense of frustration that militates against critical examination and eventual acceptance of the material presented. There is a branch of geology—I am not going to be specific, for I do not wish to hurt anyone's feelings—in which the English-speaking world for long lagged behind the Continent, mostly, I am convinced, because the only textbook in English was so appallingly difficult to read that the inquiring mind was discouraged.

Be that as it may, we can agree that the essence of good scientific writing is clarity. Although the content of a paper may be difficult to absorb, it is intolerable if an author's ignorance of the tools of his trade—and English is as much his trade as science—should further befog the unfortunate reader.

I have attempted in this article (*1*) to classify the main causes that combine to defeat the aim of clarity in geological writing. It may be that an orderly presentation of possible faults will strike a chord with those to whom a textbook of grammar or English usage is distasteful.

As far as possible I have illustrated each subhead of my classifications. I have used, for all except two examples, excerpts from manuscripts that have passed through my hands as editor in the Bureau of Mineral Resources; and my colleagues have forgiven me (*2*). Two examples, which were too good to miss, were taken from outside sources. I hope that if their authors recognize their offspring they will not hold it against me; I know very well that anybody going through my own writings could glean examples of a good many of the faults listed; and there are few of whom the same could not be said.

Ambiguity

Punctuation. (i) A common error is to omit the comma before a nondefining relative clause; although I call this an error, the normal result for the reader is ambiguity. Thus: "*Cycloclypeus* is very common in some of the limestones where it is represented by *C. indopacificus.*"

The author did not mean the clause to be defining, but the omission of a comma after *limestones* leaves the reader in doubt.

(ii) "Mildly active vents in the crator were emitting white vapour and traces of sulphur were observed in the vicinity."

Such an ambiguity is resolved as the reader proceeds, but the immediate reaction is that the vents were emitting white vapor and traces of sulfur; and the reader must go back and re-evaluate the sentence. Anything that breaks the reader's train of thought in this way is harmful: the omission of the comma after *vapour* is inexcusable.

Phraseology. Ambiguities resulting from infelicity of phraseology abound. Here is a short specimen that covers a wide field of speculation: "The copper deposits have become unpayable below the zone of secondary enrichment."

Have become poses a lot of unanswerable questions.

Obscurity

Punctuation. (i) Here is an extract, encyphered by the indiscriminate use of commas, such as no author should ever produce for a reader's puzzlement (I have disguised the names involved, which might identify the author: I do not wish to alienate my friends): "Remarks on No. 1 Bore are based on reports by Blank, on No. 2 Bore, District of Somewhere, on drillers' log notes by Dash, in a report for ABC Ltd., on bores in the So-and-so area, Mt. Such-and-such, and District of Elsewhere, on personal investigations."

(ii) Perhaps also to be recorded under punctuation, even though syntax is also involved, is the habit of stringing together large numbers of descriptive adjectives before a single noun, usually with inadequate or incorrect punctuation. Thus: "The white sand-dunes are composed of semi-consolidated and loose, white, medium-grained to fine conglomerate sized, angular to sub-rounded, fragments of foraminiferal tests, mollusca, etc., and carbonate and quartz sand."

Prolixity and involved writing. Here I take two examples from outside sources; we have never risen to these splendid heights. One of the extracts is Australian and one is American. (i) "The sharp widening in the backwardation in the Metal Exchange quotations for zinc recently illustrates the tight prompt position which has developed." (ii) "A uniformitarianistic approach to the origin of the . . . Series seems consistent with most data, and, if this philosophy is followed, some interesting environments of origin are suggested."

Cliché. Clichés are an abominable feature in manuscripts. They indicate only that the author is either unable, or too lazy, to think out the subject matter for himself. But besides the general cliché there is another allied danger against which the author must arm himself: the coining of a cliché peculiar to his own work by overusing a descriptive phrase that he is particularly fond of. The effect on the reader is the same: boredom, and a suspicion that the author is using the phrase without consideration for its precise meaning, and hence is imprecise in his thinking—for the one is the corollary of the other.

Examples are of course not possible in a short article, but perhaps I may be allowed to quote a short sentence that illustrates a cognate point: ". . . it is present as shreds and patches from 1 mm. to 5 cm. across."

The concealed quotation from *The Mikado*, even though it may well have been subconscious on the author's part, distracts the reader from the—essentially serious—de-

scription of a rock. The concealed quotation is fortunately rare in scientific work, for it has no place there.

Imprecision of expression. Clarity is precision: the constant use of vague qualifying adjectives and adverbs shows clearly that the author is pretending to a precision that his thoughts have not reached. Such work is a nightmare to edit, because the editor no more than the reader knows what the author means by such phrases as *in limited quantities, relatively shallow water,* and the rest.

Error

Punctuation. Errors in punctuation are legion; the commonest seems to be the misuse of that difficult stop, the comma. An editor—one editor, anyway—seems to spend as much time in erasing and inserting commas as in all other corrections put together.

Grammar. (i) Only a few of the many errors in elementary grammar can be quoted as examples. The unattached participle is common, and brings joy to the sophisticated: "Driving to the east for about a mile, the seam broadens." "Deep-seated rumbling was noticed while encamped on the upper slopes."

(ii) The transferred epithet occasionally pops up: "Deepest information yet obtained is from a bore."

If the information is deep, it is only in a colloquial sense!

(iii) And "number attraction"—where the verb agrees in number not with its subject but with some intervening noun—is one of the editor's daily companions: "The surface of these flat areas are usually 'crab-holey.'"

Vocabulary. (i) Most of us are so proud of our vocabulary, unfortunately, that we do not use the dictionary as we should; and hence *comprise* is misused for *constitute* (almost invariably), *mitigate* for *militate, distinctive* for *distinct,* and even *effect* for *affect.*

(ii) It is hard to convince authors, too, that absolute words cannot be qualified: *unique* and *minimize* are common victims. A more subtle snare is exposed in ". . . an extremely poikiloblastic garnet." Textural terms like *poikiloblastic* are absolute: they, too, cannot be qualified.

Nonsense. The realm of sheer nonsense is most often entered, in my experience, by authors who wish to give a spurious air of precision to a vague statement by using mathematical terms that they understand imperfectly. Four examples show this common error:

(i) ". . . a semi-vertical fault." The author meant "quasi-vertical"—or, better, "nearly vertical"—not a fault hading at 45°.

(ii) "The field is semi-elliptical in shape." But, even with the radii known, there are eight possible semi-ellipses round any given point.

(iii) "The corallites are unidirectional, but not perfectly straight." In that case, what on earth does *unidirectional* mean?

(iv) "The ratio of men at work to men . . . sick (of silicosis) . . . rose at one time to one." It is to be hoped that the rise continued.

Two points stand out from this analysis. The first is that there are two main ways of distracting the reader's attention from the scientific content of the paper he is studying: first by making it unnecessarily difficult—and frequently impossible—for him to grasp the author's meaning without first untangling a jungle of jargon and grammatical obscurity; and secondly by introducing incongruities and errors that distract his attention (one must assume that he is himself sufficiently literate to notice them). The first is the more serious; but the second may be compared with, say, a wrong entry by the cymbals during a symphony.

The other point—which rather astonishes me when I reread what I have written—is the prominence assumed by punctuation. Incorrect or inadequate punctuation is the factor common to all the major divisions I have sketched. Everyone thinks he knows how to punctuate: that is the main trouble; for it is hard to persuade intending authors that it is an art, requiring—and repaying—special study. (This paper, by the way, is punctuated according to English usage; there are several points in which it is incorrect, or at least pedantic, by American usage.)

A third point—which may be specific to Australia, although I do not think so—is that young graduates on the whole cannot write English; have never been trained in the use of English (a result perhaps of too early and too great specialization at school and university), and have not even been made aware that fluency of composition is an essential part of their professional qualifications. But that opens up such a wide vista for discussion that it may well be left to a separate paper.

Most of the points I have catalogued so far are of general application, and it is only in the examples that a specifically geological application can be seen. But there are one or two other considerations that are of special importance to the geologist.

First, there is the general question of terms that have come to us from the miner, the naturalist, and other people not primarily concerned with precision. This may affect geological writing in two ways: in the use of inherited terms that cannot be precisely defined, and in the use of metaphor.

Geology is an earthy science in more ways than one; and one need only cite a single example of a term inherited from the "practical" miner to show the difficulty. The word *ore* is in everyday use in geology, and indeed is inseparable from it; but it cannot be defined. The long and indecisive discussion that followed a recent paper by W. R. Jones (*3*) showed, not only that every contributor had a different definition to offer, but that no agreed definition which would be of general use was in fact possible. Nor is it always possible when reading a paper to understand just what the author means by the word. Attempts to restrict the meaning of a mining term—as in *ore*—or to extend it—as in *mineral*—are a common source of confusion and inexactitude.

Also, the adopted language of the geologist is rich in metaphor, some taken from other professions and some from everyday life. We talk of *beds* of sediments, of *tongues* of intrusive rock, of *flanks* of folds, and so on. Such metaphor is normally dead, but it is only too easy to make it spring to life by an unfortunate choice of words. Here is an example of a dead metaphor brought to life by the incongruous juxtaposition of another dead metaphor: ". . . consequent streams flow down wrinkles on the flanks and noses of the anticlines."

Admittedly, this is error likely to occur in all writ-

ing; but among the sciences, geology, perhaps because of its plebeian origins, seems to be particularly susceptible to it.

The geologist works, unavoidably, with incomplete data. The surface rocks on which he makes his observations are interrupted by soil, by vegetation, by human habitation, and it is a fortunate field worker to whom more than 1 or 2 percent of the total surface that he is working on is available for direct observation. From this inadequate exposure he must interpret the whole of the surface plan of the rocks; not only that, he must also extend his interpretation into the third dimension of depth, on which he can obtain very little information at all. Consequently the map and the report of the field geologist can be classified, not too contemptuously, as intelligent guesswork informed with a knowledge of precendent.

He is often right, or nearly right: that is a tribute to his skill in deduction and his assimilation of the lessons of his predecessors. But it calls for a nice accuracy in writing to be able to steer a course between the Scylla of dogmatic assertion and the Charybdis of apologetic qualification. Knowing the other fellow's difficulties, one is more lenient toward the conditional than the absolute; nevertheless, too many *maybe*'s and *possibly*'s and their like impair one's opinion of a man's work.

A third problem inherent in geological writing—and probably common to writing in all the natural sciences—hinges on the amount of routine description that must be included. Although the data presented may be of vital importance, it is somewhat difficult to avoid what one author from the Bureau of Mineral Resources has called "a deadly monotony reminiscent of a railway timetable," when describing, say, the lithology of type-sections, or successive deposits of the same mineral (there is *mineral* intruding itself in its industrial sense). Matter such as systematic descriptions of fossils can legitimately be so written: it is not meant for continuous reading or immediate absorption, but for reference; but matter written so that "who runs may read" must be written very skillfully if the reader's interest is to be maintained.

It would be idle to pretend that all the difficulties I have outlined are normally surmounted, or even that they are in all cases surmountable, either by author or by editor. They are matters of style, not of grammar, and great stylists are rare even among professional writers; and even a Gibbon might falter at the task of making routine descriptions interesting. But we can aim at perfection even though we know we cannot attain it.

Much hard work remains to be done before the scientist is persuaded that it is to his own benefit to take greater pains with his writing: that he pays for bad writing by being faced with unreadable papers by others. Maybe some day we shall see a courageous author put a footnote to a paper: "Blank's paper may be ignored, for the knowledge to be gained from it is not commensurate with the appalling effort of reading it."

In the meantime, editors will have to resign themselves to being (professionally) unpopular and misunderstood.

References and Notes

* Presented by Graham Unikel, Department of Chemistry, Stanford University.

1. Presented with the permission of the director, Bureau of Mineral Resources, Geology and Geophysics, Melbourne, Australia.
2. My thanks are due to my colleagues in the Bureau of Mineral Resources who have unwittingly provided me with my material, and who accept resignedly the return of a manuscript endorsed "Not understood: please rewrite more simply."
3. W. R. Jones, "Ore: what is it?" *Bull. Inst. Mining Met.* No. 564, p. 75 (Nov. 1953); discussion, *ibid.* No. 569, p. 333 (Apr. 1954).

A System for Testing and Increasing the Intelligibility of Technical Reports

Harry F. Arader

Institute for Cooperative Research, University of Pennsylvania, Philadelphia

The problem of making technical reports intelligible to the lay reader is of great importance. Research is increasing in volume, and more and more nontechnical persons are finding it necessary to understand the results of the expanding research effort. Project Big Ben of the University of Pennsylvania's Institute for Cooperative Research has been concerned with the problem of intelligibility during recent years. The project has evolved a system for testing and increasing the intelligibility of the many technical reports prepared for nontechnical readers.

The Big Ben system is only one of many possible systems, but it has had the advantages of trial for two years. Thanks to the various improvements that practice has suggested, I am able to report that our system actually works. Why is any system necessary for insuring intelligibility? Is it not a simple matter for the editor and the author merely to sit down quietly together and *make* a report intelligible? The answer is, I believe, *not always*. It should be, perhaps, but it is not. Why?

Let us consider the state of mind of an author who has just completed a report and is still in the heat of composition. He is the nucleus of our problem—not merely the author as author, but the author in this special, this interesting, condition.

I would not be blasphemous, but the author in this state is remarkably like God in the Book of Genesis: he has labored, he has created, now he would examine the result of his work, "And God saw everything that he had made, and, behold, it was very good" (1:31). Finally, even as God, the author is ready to rest.

The author in this particular state of mind is the heart of our problem. We must understand him, we must appreciate his condition, if we hope to solve that problem.

Let us look in upon the author who, in the sanctity of his office or laboratory, has just applied what he considers to be the finishing touches to his report. He

closes, perhaps even locks, his door, and submits to the exquisite pleasure of reading his own words. He grows a little pale and the line of his chin becomes firm under the impact of the ambitious purpose and generous scope he has bravely stated. When he reaches that section in which he sarcastically disposes of some poor scientist unfortunate enough to have engaged in a similar research effort for a different sponsor, an evil smile plays about his lips. At last, as he reads his most superb passage (the one in which he has pulled out all the literary stops at his command), he loses all control of himself and bursts into tears.

At this point the editor, who has been examining the carbon copy and has been affected somewhat differently, enters. He begins (I will not include here the vital cushions of tact and diplomacy that the wise editor would employ),

> "Look here, your report does not really fit its stated purpose and scope. You must either expand the body or make your purpose and scope less ambitious. And this sarcastic section, diabolically clever though it is, emphasizes a minor point and thus distorts your central message. By the way, this high-flying passage is ambiguous and confusing because you have used approximately five times the number of words you actually needed."

The author is now, understandably, unhappy. He is disappointed; he is hurt; perhaps he is angry. His splendid work, he believes, is threatened with emasculation. And who makes this threat? A person from that quaint never-never land, the humanities; a person whose only function on the project, the author had thought, was to provide comic relief.

Of course, *we* know that the editor is on the side of the angels. He has logic as his weapon if he is worth his salt as an editor. His warning to himself, if he is wise in his dealings with authors, has always been "Don't give them rules, give them reasons."

The author, however, in an emotional and biased state, cannot view the editor's reasons objectively. But what if a *number* of people were to tell him the same thing? What if these people were not editors, not "humanitarians," but fellow technical specialists, fellow authors? Then the author's report might be changed more easily. This is the essence of the system I have mentioned. It is, essentially, the closing of the gap between editor and author. It is the assigning to authors—for a time—the duties and responsibilities of editors. But, specifically, how does it work?

The editor must first convince management that the reporting and reviewing phases of any research effort are worth a substantial expenditure of time. The editor must be consulted on the establishment of deadlines; he must see that these are set at least one month after the date on which an author will submit what he considers his completed report.

During this month or more the editor supervises the review of the document through a number of stages. In the first—the editorial—stage, the editor or a member of his staff edits the work for mechanics of form and format. In the second—the technical review—stage, the editor designates a technical man (in the author's field and of at least the author's stature) to edit the work for technical accuracy. As stage three, the editor sets up a trial audience composed of technical specialists from fields similar to, as well as from fields divorced from, the field of the report. This group consists of 10 or 12 persons, and includes the director of the organization or his deputy. Each member of this audience receives a corrected copy of the report after it has passed stages one and two. The members of the trial audience are given at least one week to study the report and to write into it any corrections or changes that they feel are necessary. The trial audience then meets with the author to discuss the report.

The director, his deputy, or the editor acts as chairman of the meeting. First, a general question is considered: Does the report accomplish its stated purpose and stay within its stated scope? Then follows a page-by-page consideration of the intelligibility of each section, each paragraph, each sentence, and each word.

Let us suppose that the report is in the field of chemistry. If a statistician or political scientist does not understand any part of the work, he says so at that point. It may turn out that this gentleman merely did not read the particular section carefully. If this is the case, the other members of the trial audience will readily defend that section.

The instinct of the audience, we have found, is not to change any part of an author's work unless that part is actually ambiguous or misleading. If any part of the report is unsatisfactory, a good proportion of the audience will so indicate when that part is reached in the page-by-page review. The author, faced with numbers, is quick to see the error, and is quite willing to correct it.

Whenever possible, the faulty section is changed during the meeting of the audience—the new wording usually being a joint effort of the author, the editor, and the members of the audience who were most interested in the change. If the change is a complicated one, it is made later, in an informal discussion between the author, the editor, and the interested trial-audience member or members. Our experience shows that the tone of the audience is friendly. Honest effort is made toward cooperation. The author benefits from several different viewpoints and is frequently supplied useful information on content as well as on presentation.

In stage four the director or his deputy reviews the report for consistency with the overall policy of the organization. Since either the director or his deputy attends the trial-audience meeting, stage four is often combined with stage three.

The author then proofreads the masters and signs a form stating that they are accurate in every detail and have incorporated every change agreed upon during the various stages of review. An accurate, intelligible report is the result of this effort.

Of course, the success of this system depends upon

the success the editor has in convincing management of the importance of such editorial review. He has good ammunition with which to do this. It is easy to demonstrate that a research organization is not judged on the good looks or scholarly record of its investigators, but is judged on its product, information, and on the effectiveness with which that information is presented. The system that I have outlined requires control, and it costs time and effort. But the alternative can be chaos.

Sometime ago I heard of a case in which an editor had been completely bypassed: an author had sent his document directly to the contracting agency. Some months later the editor had the wicked satisfaction of learning that the report had been returned with the comment that, because of its illogical punctuation, faulty structure, and generally ambiguous presentation, it was not acceptable. As a reult of this embarrassing circumstance, I feel that that particular editor will have the full support of management in establishing such a system as I have discussed.

Technical Manuals: Their Increasing Importance to Industry and Defense

Henry E. Marschalk

Publications Branch, Bureau of Ordnance (Navy), Department of Defense, Washington, D.C.

"Graphic communications" might well be used as a second title for an article on the importance of technical manuals to industry and defense. Such a second title may appear to be very broad in scope, but I use it to help dramatize a new philosophy respecting technical manuals—a philosophy that we should embrace throughout the scientific, engineering, and technical world. Because of the extreme complexity of today's industrial machinery and weapons of defense, the instruction books we call technical manuals must be prepared according to new high standards of ready understandability. In short, our technical manuals must be engineered as carefully as the equipments they concern. That means we must expand sufficient time, thought, energy, and money on them to make them virtually faultless instruments of graphic communication. Surely this is a far cry from the concept widely entertained in the past, that technical manuals can be a kind of "cheap and dirty" afterthought in a program involving the design and production of costly and complex equipments.

Slowly and surely we are being forced by the tremendous advances we are making in our technological world to understand the need for better-written and better-illustrated instructional material. In fact, the mistakes we sometimes make because of deficient instruction manuals are reason enough for us to tackle this problem more earnestly.

In the commercial and industrial world a deficient manual program can be the direct cause of bad production, poor maintenance, customer dissatisfaction, loss of goodwill, and loss of business. It can even mean tragedy, such as took place not long ago when a commercial airliner crashed because of faulty instructions in a maintenance manual used in servicing the airplane. One paragraph of the Civil Aeronautics Board Accident Investigation Report covering this case said:

> Figure . . . illustrated the idler as a straight-designed component whereas the actual part is curved, and depicted the forward and rear push-pull rods incorrectly in their inboard and outboard relationship. Instructions . . . of the same publication referred to this figure for removal and installation purposes. From this figure, . . . , correct positioning of the idler could not be determined.

This seems to be a good example of the importance to industry, and to the lives of the public dependent upon that segment of industry, of the need for *good* and *foolproof* technical manuals. There must be countless other examples involving anything from the frustrations of a housewife when her newly purchased can opener is supplied with confusing instructions, to some major loss involving lives and costly property.

With respect to the military services, it should be obvious that mistakes can be extremely costly indeed. The mistakes may range all the way from the peacetime mishandling of small arms to performance failure, in a national emergency, of a complex guided missile system. Even during peacetime the costs of maintenance can sky-rocket when armed services personnel fail to "get the word" in clearly understandable fashion from well-written, well-illustrated maintenance and trouble-shooting manuals.

We can no longer afford to be penny wise and pound foolish in our technical manual programs. It is ridiculous to spend fabulous amounts of the public's money on defense equipments and systems, only to have a portion of those sums frittered away unneccessarily because good instructions for operation and maintenance are lacking. All too often have there been instances in which, after splendid thought had been applied to the conception, development, and production of intricate devices for defense—devices that often are referred to by the military as *hardware*—the instruction manual has been treated as a mere afterthought. We have said: "Oh, yes, perhaps we should have some kind of instructions on this," or "We've spent so much on the hardware that the instructions will have to be 'cheap and dirty.' "

We cannot afford that kind of short-sightedness any longer. It is far too costly. Today, if our intricate weapons of defense and our complex products of industry are to be well understood by those whose job it is to run and maintain them, we must apply to our instruction manuals a kind of "engineering" that, in its way, is comparable to the superb engineering on the hardware itself.

To do this we need to change our thinking about instruction manuals. We need to adopt a new philosophy concerning them. We need to realize that they are *the* vital link of communication between the engi-

neering talent that created the hardware and the technicians who operate and repair it. We must say to ourselves: "How well can this book be written? How clearly can it be illustrated? Let us make it as foolproof as we can." In the long run such a concept will pay off, although initially it may seem to be more costly.

Perhaps this simple illustration will help to emphasize the importance of good technical manuals in today's complex world. Bear in mind the second title of this article, *Graphic communications.* Bear in mind also that in any system of communication, particularly those involving electronics, such as radios and television, the slightest deficiency in a component may cause garbled transmission or reception, or may stop communication altogether. Deficiencies in such systems may be the intermittent change of value in a resistor, the burn-out of a transformer, or the breakdown of insulation in a capacitor. Similarly, in oral communication, poor organization of thoughts, deficient enunciation, and a weak voice may limit the transmission and reception of ideas.

Just so it is in graphic communication. The ambiguous wording and the illustrations that are too small or too crowded tend to reduce effectiveness. They tend to cut down on over-all clarity and they defeat or materially weaken the originally intended purpose of getting the message through.

Let us, instead, be more painstaking in our development of text, more generous in our treatment of illustrations, and let us print our material so that its effectiveness is not impaired.

From time to time I have heard that people in the scientific world are concerned about their inability to marshal support for their pet ideas and projects. Perhaps a reason for this lack of support lies in the inability of many a scientist and engineer to be understood. And why is he not understood? Because, in his oral and written and illustrated presentations he uses jargon and illustrative methods that are obscure to all except a few who possess virtually equal specialized learning.

What is the answer to this situation? In part, it would seem that in our schools, our colleges, and our universities there is need for far greater emphasis on the *how* of graphic communication. We must be taught at an early age that it is not enough to become highly capable as a scientist or engineer alone. But in addition, we must know better *how to communicate to others*—a larger, much larger field of others—those things we wish them to know about our work. We must know more about how to write lucidly; how to illustrate three-dimensionally, and how to speak so that our audience will listen and can understand.

In the preparation of technical manuals, particularly, we cannot afford to write and illustrate in a style calculated to impress others with our superior knowledge. Such treatments may be quite appropriate when our audience consists of engineers or scientists with general knowledge equal to our own. However, when our audience consists of operating personnel and maintenance technicians and, for that matter, many administrative people in the services or industry, then our problem to make things unmistakably clear is a tough one.

Until more of us learn the techniques of clear presentation, we may need to enlist the services of others whose experience and proved capability have qualified them for such work. Such specialists often provide the means for bridging the gulf between the scientific or engineering level and the technician level. These specialists have rendered great service to the armed forces and to industry. For the most part, what specialized talents there are in this field have migrated to such organizations, and the need for their services continues to grow. That is why we must now tackle the problem on a long-range basis by providing formal education along these specific lines on a scale far larger than ever before.

The leaders of the future world of science, engineering, and technology are more likely to be those men who not only are proficient in their specialized fields, but who are also proficient in their ability to set forth clearly their worthwhile ideas. If they have been unable to develop such a proficiency in themselves, they should recognize the shortcoming and seek the assistance of others who can help. In any event, they should recognize the importance to industry and defense—yes, even to civilization itself—of effective graphic communication, and particularly the importance of the technical manuals that form the link of communication between their plane of thought and that of the operating and maintenance technician who will use them?

It is only by such means that we can assure the communication of information needed to keep pace with the purely scientific and engineering strides that we now foresee.

Scientific journals are the circulatory system for the ideas of science. It is largely through them that science develops, for scientific growth is the result of cross-fertilization between laboratories and groups in different countries. One of the evil consequences of war is that it stops the flow of scientific ideas from one nation to another. And to the extent that this process is blocked the development of science is definitely retarded.—RAYMOND B. FOSDICK.

News and Notes

Current Thoughts in the History of Science

A truism now widely believed is that science is more genuinely understood by one who is familiar with its historical background and, conversely, that history which fails to include an account of scientific development is but half the story—and perhaps not the better half. But things taken for granted easily may be overlooked; and a report on recent work in the history of science may serve as a reminder of the part that this relatively young discipline plays in relation to the traditional subjects of which Hedodotus and Thales are said to be the founding fathers.

One might expect that a paper on "The true place of astrology in the history of science" would be of interest primarily to the antiquarian; and yet when Lynn Thorndike (Columbia University) spoke on this topic at the annual meeting of the History of Science Society in New York, 29–30 Dec. 1954, the result was a lively discussion of the aims of modern science. The main thesis of Thorndike's paper was that the basic characteristics of the science of our times can be found in medieval astrology. The detection of regularities was then, as now, one of the chief aims; the tendency to fuse all sciences into one was just as pronounced then as today; the use of mathematical method was as much a part of Kepler's astrology as of his optics; and the principle of causation and the hypothesis of universal natural law were clearly in evidence in the medieval astrological belief that it was appropriate for higher bodies (the heavens) to govern the lower (including man), or, in other words, for celestial virtues to generate terrestrial results. Any comparison of medieval astrology with modern science must, of course, distinguish between the legitimate and the spurious, between natural and judicial astrology; and such distinction was clearly made during the Middle Ages.

In commenting on this paper, Francis R. Johnson (Stanford University) pointed out that, if astrology held that the stars incline but do not compel, this is not very different from the modern doctrine of the inheritance of a tendency toward a disease. Comment from the floor suggested that a universal principle, such as "God wills it," from which all phenomena are deducible as consequences, may in itself, by explaining everything, explain nothing; and a fragmentarian view of science may well be preferred to the assumption of a broad general law that cannot be tested.

Comparison of the science of earlier days with that of today was made also in other papers of the session devoted to *Medieval and Early Modern Science*. Francis J. Carmody (University of California) presented "Notes on Thabit b. Qurra's astronomical works," in which revisions in data from Ptolemy and early Arabic writers were indicated. It was on the basis of the accumulated mass of stellar observations that Thabit was led to accept the now outmoded theory, known as trepidation, that the precession of the equinoxes was subject to an oscillatory variation.

In a paper on "William Gilbert and experimental science," Duane H. D. Roller (University of Oklahoma) pointed out that the word *experiment* meant different things to Gilbert and Galileo. For Galileo, who was interested in a theoretical structure that permitted extensive scientific prediction, the word sometimes designated what now should be referred to as a "thought experiment." In spite of the single-mindedness of the author of *De magnete*, Gilbert was less concerned with theoretical unity than with specific experiences. (Although he accepted the diurnal rotation of the earth, he seems not to have been concerned with the other Copernican motions.) And yet in his work one discerns two types of "experiments," those which he had carried out for himself, and others which clearly were not intended to be performed. In view of the scanty knowledge concerning Gilbert's life, Roller called attention to certain persistent biographical errors.

The meeting of the History of Science Society opened with a session on *Science in Antiquity*; here, too, there was an emphasis on relationships to modern counterparts. This was most pronounced in the "Remarks on ancient psychopathology" by I. E. Drabkin (City College of New York). It was remarked that Greek science held mental derangement to be a disease which was to be approached rationally and treated naturally. The role of heredity, the effects of body constitution (especially through humoral imbalance), and the importance of psychotherapeutic and somatic measures were appreciated. Violent measures, equivalent to shock treatment, generally were shunned for gentle and sympathetic approaches.

A different way of relating ancient and modern thought was suggested by Mark Graubard (University of Minnesota) in a demonstration of the use of "Ancient instruments in teaching modern science." Graubard displayed devices that he had constructed for use in classes to stimulate interest in the science of remote periods. He reported that nothing serves so effectively to awaken in the blasé college student a wholesome respect for the work of Ptolemy as the awareness that modern mechanical astronomical models, including planetaria, are without exception Ptolemaic rather than Copernican.

An unfamiliar facet of Ptolemy's work was presented in the opening paper of the meeting by William D. Stahlman (Massachusetts Institute of Technology), speaking on "Ptolemy's post-Ptolemaic cosmology." Stahlman pointed out that in a treatise on the *Hypotheses of the Planets*, composed after the better known *Almagest*, Ptolemy had advocated, presumably under Platonic influence, the use of "sphere pieces" in a physical representation of the geometric scheme of cycles and epicycles. The use of complete spheres in the celestial mechanism was felt to be a violation

of the principle of parsimony; and by the same type of reasoning, Ptolemy postulated for the ether an east-to-west motion in which the planets shared, thus simplifying the necessary celestial gear mechanism.

Science and the French Revolution was the theme of a joint session with the American Historical Association. L. Pearce Williams (Yale University), speaking on "The organization of science during the Revolution," pointed out that France had been far behind England in encouraging inventiveness, and that when such a program was undertaken by decree of the Assembly in 1790, the main emphasis was on agriculture. The provincial academies had become inactive, following 1789; but by 1805 there were at least 70 French agricultural societies. Napoleon appreciated more fully the importance of industrial enterprise, but developments in this direction were obstructed by his international policies.

Another aspect of scientific organization was presented by Henry Guerlac (Institute for Advanced Study) in a paper on "The anatomy of vandalism." Guerlac analyzed factors underlying the attack on the Académie des Sciences which led to its dissolution in 1793, at a time when the prestige of the Jardin du Roi (later the Musée d'histoire naturelle) was in the ascendant. It was indicated that the Académie, which stressed mathematical and physical science, had been attacked by Rousseau, an enemy of formal science, and later by Marat; the science of the Jardin, on the other hand, was more compatible with Romanticism and the Cult of Civic Virtue which Robespierre had embraced. In other words, the Jardin, rather than the Académie, was the guardian of "approved science." Then, too, the more conservative wing of the Académie, of which Lavoisier was a guiding spirit, had triumphed over the more liberal faction of which Condorcet was the spearhead, with the consequence that the affairs of the Académie remained closely linked with the royal government. The Jardin, during the early years of the Revolution, included a significant number of younger men who, carrying the institution toward the Left, had placed themselves under the Assembly as the Jardin des Plantes.

In the third paper of the session, Paul Beik (Swarthmore College) presented "Some reflections of the revolution in political science." Comparing Robespierre's suspicion of government by the majority with Condorcet's faith in representative government, Beik concluded that Condorcet was right in the wrong century. In commenting on the papers of this session, Pierre Donzelot, permanent representative of French universities in the United States and formerly president of the Jardin des Plantes, pleaded for a sympathetic judgment on the closing of the Académie, attributing this error to stress during a period of crisis, rather than to antiscientific bias.

The fourth session, on *American Science in the Colonial Period,* centered about Puritan and Quaker faith and scientific activity. Margaret Denny (University of Rochester), in a paper on "The science of two Puritans of Massachusetts Bay," noted similarities between the duties of a fellow of the Royal Society and the obligations of a Puritan under the Doctrine of the Covenant. The Puritan training of Cotton Mather and Paul Dudley, with its emphasis on zealous cooperation and the idea that the study of nature is an aid to religion, was appropriate to their later election to the Royal Society, for the fellowship of saints was not so different from that of scientists. However, church rivalry, personal antipathy, and strong individualistic tendencies made the lines of communication with England stronger than those between the Colonists themselves.

Brooke Hindle (New York University) made a somewhat corresponding analysis of "The 'Near' Quakers of Philadelphia and the American Philosophical Society." Hindle noted that it was not exclusively the moderate or Puritan background that furnished fertile ground for the growth of science. The Quakers, with their rational pursuit of knowledge, and more especially through their concern for things rather than words, were among the radical sects with scientific proclivities; and it was peripheral Quakers, former Quakers, and "near" Quakers who were important in the formation in 1769 of the American Philosophical Society, even though the Quakers were at the time a minority of roughly 13 percent in Philadelphia. Perhaps, too, Quaker inhibitions played a part here, for the wealthy Philadelphians evinced but little interest in science.

The picture of Quaker influence in science was continued by Frederick B. Tolles (Swarthmore College) in an account of "The scientific activities of James Logan." Tolles lauded especially Logan's contributions to botany and mathematics, calling him "the most distinguished American scientist before Franklin." Logan made, independently of Mather, observations on sexuality in plants; and he submitted to the Royal Society a paper in which he simplified the calculations of Huygens.

In commenting on the papers of the session, I. Bernard Cohen (Harvard University) called attention to the fragmentary state of knowledge of scientific activity in other colonial religious communities, such as that of the Huguenots. Cohen pointed out also that Franklin had been able to make an easy transition from the society of Boston to that of Philadelphia; but since he was so successful also in adjusting to British and French modes of life, this may simply be a mark of his versatility. Noting that Quaker empiricism did not lead to deep mathematics or to a truly great scientific discovery, Cohen conjectured that perhaps a great scientist is a freak of nature rather than a product of his environment.

The meeting of the History of Science Society closed with the annual dinner. Dorothy Stimson, president of the society, presided and introduced the speaker of the evening, Raymond J. Seeger, assistant director of the National Science Foundation. In an address entitled "On the history and philosophy of science," Seeger called attention to characteristics of the separate disciplines designated by the nouns in

the title and then to the functions of the history and philosophy of science. He pointed out that the National Science Foundation is prepared to encourage and support projects in the fringe area between natural science and social science, including in particular the field of the history of science. This assurance was a fitting note on which to close the meeting of the History of Science Society, for in a general sense the theme of the meeting had been the part that the history of science can play in the understanding of modern science.

CARL B. BOYER

Brooklyn College, New York

Science News

The Joint Congressional Committee on Atomic Energy has established an eight-member citizens' panel to help **expedite the peaceful uses of nuclear energy.** Robert McKinney, editor and publisher of the *Santa Fe New Mexican*, will be chairman of the group of scientists, industrialists, and civic leaders. He is a former Assistant Secretary of the Interior, and during World War II he served as a lieutenant in the U.S. Navy's Bureau of Ordnance. Other panel members are Ernest R. Breech of Detroit, chairman of the Ford Motor Co.; George R. Brown of the construction firm of Brown and Root, Houston, Tex., chairman of the Texas Eastern Transmission Co., and a former member of President Truman's Materials Policy Commission; Sutherland C. Dows of Cedar Rapids, Iowa, chairman of the Iowa Light and Power Co.; John R. Dunning, physicist and dean of engineering, Columbia University, and director of the Oak Ridge Institute of Nuclear Power; Frank M. Folsom, president of the Radio Corporation of America, New York; T. Keith Glennan, president of Case Institute of Technology, and former member of the AEC; and Samuel B. Morris, general manager and chief engineer of the Los Angeles Dept. of Water and Power, and chairman of the atomic energy commission of the American Public Power Association.

The panel, which may be enlarged, will have four main duties:

1) To appraise the present and future impact of all aspects of the development of atomic energy on our way of life, economy, industry, and natural resources, including the effect upon employment. While this appraisal will be concerned principally with the peaceful applications of atomic energy, it must take into consideration its military applications as they affect or concern peaceful uses.

2) To consider the effects of the application of atomic energy upon industries abroad. Although the study will be concentrated upon United States industry and economy, it must take into account the interlocking effects that such development and application abroad might have on our own economy and industries.

3) To study the activities of the Atomic Energy Commission as they affect the foregoing, both in the AEC programs directed toward developing peaceful uses of atomic energy and in the AEC role as the regulatory agency.

4) To recommend to the Joint Committee any legislative or policy actions needed to speed the development, under both Government and private auspices, of peaceful uses of atomic energy.

The National Academy of Sciences will undertake a broad appraisal of the **effects of atomic radiation** on living organisms and will seek to identify questions upon which further intensive research is urgently needed. This project was announced on 8 Apr. by Detlev W. Bronk, president of the National Academy. The study has been assured financial support by the Rockefeller Foundation, which has already made an initial grant for planning purposes.

Lewis L. Strauss, chairman of the U.S. Atomic Energy Commission, has assured the Rockefeller Foundation and the National Academy of the commission's full cooperation. Extensive investigation of radiation effects has been sponsored by the commission since its establishment, and data obtained from this research will be made available for the new study. Expenditures for the AEC's biological and medical program have totaled more than $165 million since 1950, and more than half of this sum has been expended for research on the effects of atomic radiation on living organisms.

Wide differences of opinion exist regarding the nature and degree of human hazards involved in the use of atomic energy, and Bronk said that the academy welcomed the opportunity to make a disspassionate and objective effort to clarify the issues, which are of grave concern, as well as of great hope, to mankind.

The academy will appoint a committee of scientists, supported by an appropriate staff, to carry out the study. Some of the committee members will have an intimate contact with the work of the Atomic Energy Commission and will, therefore, be particularly well informed concerning the background of the problems under consideration. The investigations, deliberations, and ultimate reports of the committee would deal with all phases of the biological effects of the increasing use of atomic energy. The study will collect and evaluate scientific information bearing on these problems; formulate whatever conclusions are, in its judment, warranted by the available evidence; identify problems that require further research; and initiate such investigations. A searching appraisal of the state of medical knowledge regarding therapy and protection is also envisioned. Finally, the academy will evaluate the availability of information to scientists, physicians, and the general public.

Harvard University has received a grant of $132,000 from the National Science Foundation for the construction and operation of a 60-ft parabolic **radio-telescope antenna** to be located at the George R. Agassiz Station of the university observatory. The radio-telescope installation will be able to receive and plot

radiations from neutral hydrogen atoms in interstellar space. It overcomes limitations of optical telescopes, which, with the aid of photography, must confine astral scanning to areas only slightly beyond the visual wavelengths of light. It penetrates the cosmic dust clouds and water vapor of the earth's atmosphere that seriously hamper or cut off optical viewing. Donald H. Menzel, director of the Harvard College Observatory, estimates that construction of the radiotelescope, which will be the largest university-operated one in the United States, will be completed in about 1 yr. The new telescope will supplement a 24-ft radiotelescope (also sponsored by NSF) that has been operating at the Agassiz Station since 1953.

The new antenna will permit more accurate plotting of the sources of radio energy than the 24-ft antenna, because its beam width will be about 0.5° compared with the older antenna's beam width of 1.5°. Therefore it is expected that investigations of the fine details of the spiral structure of the Milky Way system, as well as studies of "radio stars," can be carried out.

Physicists have known since the 1930's that the neutral hydrogen atom produces microwave radiation. H. C. van de Hulst of Leiden, Holland, theorized in 1944 that the radiation resulting from changes in the relative orientation (parallel to antiparallel) of the magnetic axes of the proton and the electron in the hydrogen atom—which had already been demonstrated in the laboratory—would also occur in space. He placed the wavelength of radiation at 21 cm. Although such changes would probably be relatively rare occurrences, Van de Hulst reasoned that there were enough neutral hydrogen atoms performing at any one time to permit detection of the radiation by sensitive receivers. The radiation on the 21-cm band was first observed by Ewen and Purcel in 1951 and very shortly thereafter by J. H. Oort, C. A. Miller, and Van de Hulst in Holland, and by Australian radio astronomers. The Dutch astronomers then began radio exploration of the spiral structure of the galaxy.

The radiotelescope permits the blending of the results of electronic and optical observations. The radiotelescope is an extension to astral explorations. Optical astronomy is confined to about 5 octaves of the electromagnetic spectrum. The radiotelescope adds 12 octaves to the observable electromagnetic spectrum. Its range in wavelength is from less than 0.5 in. to 100 ft.

Astronomers are generally agreed that there are at least two varieties of stellar "broadcasting stations." One is the relatively motionless hydrogen gas that makes up most of the matter in interstellar space and an estimated 10 percent of the Milky Way. The other is known as a discrete radio source or radio star that appears to be associated with highly turbulent gas masses.

The radiation signals are received by the parabolic reflector and focused on a small dipole that picks up the signal and converts it to electric energy. The resulting current is fed into a receiver where it is amplified and transferred graphically to a pen moving across a sheet of paper. The graphs reveal the shape and extent of particular gaseous spiral arms in the Milky Way system or the amount of neutral hydrogen along a particular line-of-sight. The result is in effect a new mapping of space, giving electronic form to outer and hitherto optically unseen regions.

Studies of sites in space where new stars may now be forming make up one of the major research programs now under way at the Agassiz Station. These sites are small and very dense clouds of cosmic dust, marked by concentrations of neutral hydrogen. Many astronomers believe these clouds to be likely sources for the birth of new stars.

Another of the Agassiz Station research projects concerns planned regional surveys of the spiral structure of our galaxy. Some 200 galaxy "centers" are now under study. Two-thirds of the studies are concerned with the relationships betwen cosmic dust and gas, the remainder with spiral structures.

Observations on the **first living coelacanth** are reported by James Millot, director of the Institut de Recherche Scientifique de Madagascar, in the 26 Feb. issue of *Nature* (p. 362). The eighth *Latimeria* to be captured since 1938, the specimen was caught at a depth of 255 m on 12 Nov. 1954, about 1000 m offshore opposite Mutsamudu jetty. The fish, a near-adult female, weighed 41 kg and measured 1.42 m in length.

Biological observations of the previous specimens were impossible because the fishermen either battered the fish to death with oars or killed it with harpoons and knives to prevent it from struggling and making it difficult to hoist the fish into the narrow pirogue. The fishermen also feared to tow the fish into harbor because a shark or barracuda might take the fish and lose them the reward. Because of these difficulties, a double reward was offered for a live coelacanth.

The eighth *Latimeria* was placed in a sunken small boat off the end of the jetty about 11:30 P.M. It lived there for nearly 24 hr. It was observed that the fish was very dark grayish-blue in color and that the fins had clearer gray-blue reflections. "The greenish-yellow luminescence of its eyes was very pronounced. . . ." The fish swam slowly by "curious rotating movements" of its pectoral fins,

> . . . While the second dorsal and anal, likewise very mobile, served together with the tail as a rudder. After daybreak it became apparent that the light, and above all the sun itself, was upsetting the animal very much, so several tent canvases were put over the boat to serve as some kind of protection. But despite this precaution and the more or less constant renewal of the water, the fish began to show more and more obvious signs of distress, seeking to conceal itself in the darkest corners of the whaler.
>
> At 14:45 hr. it was still swimming feebly; but at 15:30 hr. it had its belly in the air and only the fins and gill-covers were making agonized movements.
>
> It was then covered with a sheet and taken immediately to the hospital. There was not a scratch on it, apart from a tiny incision in the centre of the anterior part of the floor of the mouth made by the

fisherman when recovering his hook. Altogether, it was in remarkably good condition, without any rupture of the viscera or suffusions of blood. . . .

Chemical and histological investigations could be made under the best possible conditions on perfectly fresh tissues. . . .

Two principal conclusions emerge from the corroborated statements made by local observers and by myself: (1) the extreme photophobia of *Latimeria*—the sunlight seemed literally to hurt it; (2) the exceptional mobility of the pedunculate fins, correlated with the wealth of musculature which is revealed by anatomical studies. The pectorals, in particular, can move in almost any direction and show themselves capable of assuming practically every conceivable position.

Millot feels that the only way to keep a coelacanth alive for a longer period is by construction of a large cage that could be kept submerged at 150 to 200 m most of the time but could be hauled to the surface for short periods of observation.

Scientists in the News

Norman Dott, professor of surgical neurology, Edinburgh, will deliver the annual Hughlings Jackson memorial lecture, which will be held this year on 13 May in the Montreal Neurological Institute. He will discuss the common features in brain displacements by tumors, by hemorrhage, and by violence.

Karl Lark-Horovitz, head of the department of physics at Purdue University, Lafayette, Ind., is discussing "Irradiation physics of semiconductors" as a Sigma Xi national lecturer at a number of colleges, universities, and research laboratories during this month and next.

H. Limburg, professor of obstetrics and gynecology at the University of Hamburg, Germany, delivered a lecture on "The early diagnosis of cancer in the female genital tract" on 7 Apr. at the University of Texas Medical Branch.

Clifford G. Fick, who has been with General Electric since 1925, has been appointed manager of the research liaison services section of the Research Laboratory at Schenectady. In his new capacity, Fick will direct the activities of the laboratory's liaison scientists, a group responsible for maintaining a two-way flow of information between the Knolls Atomic Power Laboratory and the operating components of the company.

C. Harold Berry, Gordon McKay professor of mechanical engineering at Harvard University and a leader in the field of heat engineering, will retire this summer. Prior to joining the Harvard faculty in 1928, Berry served as assistant to the chief engineer of the Detroit Edison Co. and as associate editor of *Power* magazine. For 25 yr he was a member of the Power Test Codes Committee of the American Society of Mechanical Engineers and chairman of the Individual Committee on Test Codes for Steam Turbines. Recently he was appointed to a committee advisory to the director of the National Bureau of Standards in the field of heat and thermodynamics.

Berry has been a frequent contributor to various technical publications. His book, *Flow and Fan* (1954), dealing with ventilating systems, is an outgrowth of a series of lectures presented at the Harvard School of Public Health. He has served as adviser and expert witness in damage suits and patent infringement litigation involving mechanical and engineering equipment.

In 1912 Berry received the M.E. degree from Cornell University and in 1916 the M.M.E. He was an instructor and assistant professor of heat power engineering at Cornell from 1913 to 1918. During the first world war he was with the Inspection Division, U.S. Army Ordnance Department, as a civilian.

Chauncey D. Leake, professor of pharmacology and toxicology, and of the history and philosophy of medicine and public health at the University of Texas Medical Branch, Galveston, has resigned his position as executive director of the Medical Branch, which he has held since 1942. He will continue to act as director until his successor is chosen. Leake, who is a member of the AAAS board of directors, plans to devote more of his attention to pharmacology and the history of medicine.

During Leake's administration the number of students and staff doubled, endowments were brought to more than $1 million, and the value of land and buildings at the Medical Branch quadrupled. Also during this period the number of approved residencies in the John Sealy Hospital was expanded to 18, the number of residents in specialty training increased from 20 to 150, and the number of beds in the hospital available for teaching purposes rose from 400 to 930. In addition, five clinical departments were placed on a full-time basis and 16 special research laboratories were established. Recently the James W. McLaughlin fellowship program in infectious diseases and immunity was developed. Leake also founded *Texas Reports on Biology and Medicine* in 1943.

Chester A. Arents, coordinator of research and professor of mechanical engineering at Illinois Institute of Technology since 1947, has been appointed dean of West Virginia University's College of Engineering. He will succeed **R. P. Davis,** who will retire in June after 44 yr of service.

For the invention of the transistor, **Walter H. Brattain** of Bell Telephone Laboratories and **John Bardeen,** professor of physics and electrical engineering at the University of Illinois since 1951, have received John Scott medals and $1000 premiums from the board of directors of City Trusts of Philadelphia, which administers the award.

Brattain has been associated with Bell Laboratories as a research physicist since 1929. Initially his work was concerned with thermionics, particularly the study of electronic emission from hot surfaces. He also car-

ried on investigations of frequency standards, magnetometers, and infrared phenomena, and for about 20 yr has specialized in the physics of semiconductors.

Bardeen was an assistant professor of physics at the University of Minnesota from 1938 to 1941 and served with the Naval Ordnance Laboratory as a physicist for 4 yr during World War II. On joining Bell Laboratories as a research physicist in 1945, he was primarily concerned with theoretical problems in solid-state physics, including the study of semiconductors. In the course of this work, he and Brattain invented the point-contact transistor.

Arthur Knudson, professor and chairman of the department of biochemistry at Albany Medical College since 1921, will retire as chairman on 30 June but plans to continue in academic work for several years. An expert on the metabolism of cholesterol, which has gained prominence in recent years because of its association with arteriosclerosis, Knudson is well known for his studies on vitamin D and its formation by ultraviolet and other irradiation. He has also carried out experiments on biochemical effects produced by exposure to diathermy, a method of producing artificial fever. On leave of absence from the medical college from 1951 to 1953, Knudson gained wide recognition for his work in the establishment of an experimental nutrition laboratory in Bangkok, Thailand.

The 1955 Research Medal award of the Columbia University Committee on Dental Education has been presented to **Joseph L. T. Appleton,** dean emeritus of the Thomas W. Evans Museum and the School of Dentistry of the University of Pennsylvania "in recognition of [his] lifetime of scholarship and research in dentistry."

Gordon H. Seger, formerly executive officer of the National Cancer Institute, has been appointed chief of extramural programs at the National Institute of Neurological Diseases and Blindness in Bethesda, Md. His new duties involve planning and supervision of the institute's programs of research grants, research fellowships, training grants, and traineeships. He succeeds **Edward P. Offutt.**

Carl N. Shuster, Jr., formerly of Rutgers University, has been appointed director of the Marine Laboratory, department of biological sciences, at the University of Delaware at Lewes. He replaces **L. Eugene Cronin,** who resigned to accept the directorship of the Chesapeake Biological Laboratory at Solomons Island, Md.

Wilson D. Leggett, Jr., rear admiral, USN, chief of the Navy's Bureau of Ships, was appointed vice president of engineering of American Locomotive Co. on 31 Mar., when he retired from the Navy. Under Leggett's leadership, nuclear propulsion for ships became a reality with the completion of the submarine *Nautilus.* Other achievements during his 20-mo tenure as bureau chief include construction of the Forrestal-class of aircraft carriers and many other new combat ships.

Leggett was formerly commanding officer of the U.S. Naval Engineering Experimental Station, Annapolis, Md. He served overseas during both world wars and was particularly commended for his skillful organization and supervision in the Pacific theater of an effective repair program for battle-damaged fleet units, a program that enabled them to return to duty in minimum time.

In an earlier assignment with the Bureau of Engineering, Leggett was in charge of internal-combustion engine development for all purposes and complete machinery installations for submarines and motor vessels. He was instrumental in initiating and completing a program of diesel-engine development that completely freed the Navy from dependence on German designs and made possible the diesel-electric propulsion that proved to be so successful in World War II.

Meetings

The 47th annual meeting of the **Air Pollution Control Association** will be held in Detroit, 22–26 May. Air pollution control experts from the United States and Canada will report on the latest findings in smoke abatement, air pollution measurements, odor control, and new developments in abatement equipment.

Other technical sessions include "The effect of pollutants on plants and animals" by D. M. Thomas of Stanford Research Institute, and "Meteorological effects" by Maynard Smith, consultant for the Brookhaven National Laboratories. For information write the association headquarters at 4400 Fifth Ave., Pittsburgh 13, Pa.

The 22nd Legislature of the State of New Mexico has passed a resolution, introduced by Senator W. C. Wheatley, to commend the AAAS, the University of New Mexico, and the New Mexico Institute of Mining and Technology for their various contributions to the **International Arid Lands Meetings** that are to be held in Albuquerque and Socorro, 26 Apr.–4 May. The resolution points out that water is the most important resource in New Mexico and in the Southwest as a whole, and indicates that the forthcoming meetings, which will bring together arid lands experts from all parts of the world, may well have results of great value. Governor John F. Simms signed the joint resolution on 21 Mar.

Members of the Naval Research Reserve will discuss **research methodology** in a seminar at the Ohio State University and Battelle Memorial Institute, 13–25 June, under the sponsorship of the Office of Naval Research. The program is planned to encourage maximum audience participation. Talks by skilled investigators will be alternated with small-group discussions in which NRR members will explore topics of interest in more detail. In final sessions, the groups will meet together to summarize their separate findings and question the original speakers further.

Basic concepts in research in the physical, biolog-

ical, and social sciences will be considered, as well as their applications to naval problems. Attention will be directed particularly to correlations between methods and such factors as number and kinds of variables, degree of control, precision, of measurement, and the historical development of the science.

The 5th annual **Conference on Diseases in Nature Transmissible to Man,** under the auspices of the Texas State Health Department, will be held at the University of Texas Medical Branch, Galveston, 22–23 Apr. Visitors will be in attendance from the United States, Mexico, and Canada. Local arrangements are being made by Morris Pollard, professor of preventive medicine and public health.

The 87th annual meeting of the **Kansas Academy of Science** will be held 5–7 May at the University of Kansas. The history of science in Kansas, water, and scientific hobbies will be featured lecture topics at the general sessions. These subjects will be discussed respectively by Robert Taft, professor of chemistry at the University of Kansas and editor of the *Transactions of the Kansas Academy of Science*; Harold E. Thomas, ground-water geologist of the U.S. Geological Survey, Salt Lake City, Utah; and A. C. Carpenter, president of the academy.

The 9th annual meeting of the **American Electroencephalographic Society** will take place at the Palmer House in Chicago, 10–12 June, immediately preceding the annual meeting of the American Neurological Association. In addition to the regular sessions, there will be two symposiums; one on microelectrodes under the chairmanship of Herbert H. Jasper, and another on clinical EEG interpretation (head injuries) under the chairmanship of Frederic A. Gibbs.

Education

The **Wayne University Computation Laboratory** has announced summer courses that will take place 6 June–25 July. Four major areas are to be covered: electronic computers and their business and engineering applications; automatic data processing; mathematical programing of management problems; and numerical methods and advanced programing techniques. In addition to the regular staff, visiting experts in the respective fields will conduct the lectures, discussions, and workshops. For information write to A. W. Jacobson, Director, Computation Laboratory, Wayne University, Detroit 1, Mich.

The University of Florida, North Carolina State College, Virginia Polytechnic Institute, and the Southern Regional Education Board are jointly sponsoring a series of cooperative **summer sessions in statistics.** The first of these cooperative graduate sessions took place during 1954 at V.P.I.; the second will be held at the University of Florida 20 June–29 July. A session is scheduled to be held at North Carolina State College in 1956, and another at V.P.I. in 1957.

The summer courses are designed to carry out a recommendation of the Southern Regional Education Board's Advisory Commission on Statistics, on which the three institutions initiating the program are represented. The sessions will be of particular interest to (i) research and professional workers who want intensive instruction in basic statistical concepts and who wish to learn modern statistical methodology; (ii) teachers of elementary statistical courses who want some formal training in modern statistics; (iii) prospective candidates for graduate degrees in statistics; (iv) graduate students in other fields who desire supporting work in statistics; and (v) professional statisticians who wish to keep informed of advanced specialized theory and methods.

Each of the summer sessions will last 6 wk, and each course will carry approximately 3 semester-hours of graduate credit. The program may be entered at any session, and consecutive courses will follow in successive summers. This work in statistics may be applied as residence credit at any one of the cooperating institutions, as well as at certain other institutions, in partial fulfillment of the requirements for a master's degree. The catalog requirements for the degree must be met at the degree-granting institution. Each doctoral candidate should consult with the institution from which he desires to obtain the degree regarding the applicability of the summer courses in statistics.

The total tuition fee will be $35; the holder of a doctorate degree, upon acceptance, may register without the payment of any fee. Inquiries should be addressed to Prof. Herbert A. Meyer, Statistical Laboratory, University of Florida, Gainesville.

A 2-yr training program in which 450 technicians participated has recently concluded at the prosthetics training center of the **University of California.** Under this program, orthopedists, therapists, and artificial-limb makers were instructed in the latest techniques of making and fitting artificial limbs as perfected in a research program carried on in U.C.L.A.'s engineering department since 1945 in cooperation with the Veterans Administration and the National Research Council.

Since completion of the training program, the prosthetics training center has been reorganized into three separate projects: (i) the continuation of artificial-limb research under Craig Taylor; (ii) research on special problems in fitting growing children with artificial limbs; (iii) the educational aspects of artificial limbs, including teacher training and literature publication.

St. John's University of New York has announced the scheduling of a science-pharmacy building at the university's new suburban campus in Hillcrest, Queens. Present plans call for a $2.5-million structure, with construction to commence when about half this amount has been raised. The building will include a general science library, lecture halls seating 240 students, lecture rooms seating 90, and many student and faculty research laboratories.

In the Laboratories

The Atomic Energy Commission has announced that an agreement has been signed by the commission and by the **Baldwin-Lima-Hamilton Corp.** of Philadelphia, Pa., and the **Denver and Rio Grande Western Railroad Co.** of Denver, Colo., for a study of a nuclear-powered reciprocating engine under the AEC's industrial participation program. The companies will weigh the engineering, technical, and economic aspects of an engine of this type and make recommendations concerning the role of industry in carrying out its development. There are many potential applications of a power generating unit of the kind to be studied, including locomotive propulsion. This is a 1-yr project that will be financed entirely by the companies.

Associated Chemical Engineers, a consulting partnership with offices at 5118 Beeler St., Pittsburgh, Pa., has been established by R. B. Beckman, L. N. Canjar, R. R. Rothfus, H. L. Toor, and D. H. Archer of the department of chemical engineering at Carnegie Institute of Technology.

The consulting firm is a full-time, integrated organization serving the petroleum, chemical, and allied process industries. It will undertake short- or long-range problems in the fields of research, evaluations, and management assistance.

North American Aviation is developing the SM-64 Navaho long-range, surface-to-surface guided missile, the U.S. Air Force has disclosed. Details of the missile and its performance were not revealed.

Work on the SM-64 Navaho has been underway for some time in North American's Missile and Control Equipment operations (MACE) at Downey, Calif. Started in 1945, North American's MACE operations are engaged in all major phases of missile airframe design, rocket engine propulsion, and automatic guidance and control equipment.

The U.S. Atomic Energy Commission has approved a study of reactor technology by **Combustion Engineering, Inc.,** under the Industrial Participation Program. Combustion Engineering, manufacturer of steam generating units, will study design and evaluation of large reactors for central station power production and of small size reactors for special applications, design of reactor fuel elements, and development of fuel element fabrication processes. Work has started on new $7-million facilities at the company's Chattanooga, Tenn., division.

Convair Division of General Dynamics Corp. has engaged 14 scientists as consultants for the consideration of special problems in the development of military aircraft and missile systems for which Convair is responsible to the armed services. The group also will study problems of basic nuclear research and the industrial applications of nuclear power.

John J. Hopkins, chairman and president of General Dynamics, did not mention the type of "missile systems" the group will work on. It is known, however, that Convair is at work on Atlas, an intercontinental guided missile.

The scientists engaged are Hans A. Bethe, Cornell University; Kenneth M. Chase, University of Michigan; Charles L. Critchfield, University of Minnesota; Mark M. Mills and Edward Teller, University of California; John A. Wheeler, Princeton University; Robert F. Mehl, Carnegie Institute of Technology; Frederick Seitz, University of Illinois; Lan Jen Chu, Massachusetts Institute of Technology; Richard Courant and Peter D. Lax, New York University; Milton S. Plesset, California Institute of Technology; L. Whipple, Harvard University; and Theodore von Karman, chairman of advisory group on aeronautical research and development, North Atlantic Treaty Organization.

Miscellaneous

The Smithsonian Institution announced 29 Mar. that for a limited time it is offering for sale copies of nine numbers of the **Harriman Alaska Expedition Reports (1910–1914)**. These large cloth-bound volumes are being offered for $1.25 each (postpaid). Requests and remittances should be sent to the Editorial and Publications Division, Smithsonian Institution, Washington 25, D.C. The volumes available are on glaciers and glaciation; geology and paleontology; cryptogamic botany; insects; crustaceans; nemerteans; bryozoans; land and fresh-water mollusks; hydroids; monograph of the shallow-water starfishes of the North Pacific coast from the Arctic Ocean to California.

Book News, published by Stechert-Hafner, recently carried the following announcement concerning an **exhibit of French scientific books.**

> The French Cultural Services Division of the French Embassy, with the cooperation of leading French publishing firms, has organized an exhibit of the most important scientific and technical books published in France since 1949, an exhibit which is available upon request to all interested libraries and institutions. The purpose of the exhibit is to familiarize American scientists with the work being done in France and to "contribute to a fruitful understanding for the greater benefit of science in general."
>
> The first Exhibit was officially opened at New York University on February 11, and the schedule is already full until May. Nearly 400 books on pure and applied science are packed in specially built wooden crates, and these will be circulated among all university centers willing to display the exhibit; then the books will be divided among the exhibitors. Three weeks are scheduled for each institution, but a shorter or longer period can be arranged for.
>
> In the printed catalogs, available to exhibitors, the books are divided into two main groups—science and technology—and then arranged alphabetically under the usual subject headings. . . . The French Cultural Services will reimburse the shipping costs. For further information, please write to M. Pierre Donzelot, Cultural Division of the French Embassy, 972 Fifth Ave., New York 21.

Book Reviews

The Fifth Amendment Today. Erwin N. Griswold. Harvard Univ. Press, Cambridge, 1955, vi + 82 pp. Cloth, \$2; paper, 50¢.

We are a nation seriously frightened by the threat of communism. We recoil against inquisitional techniques of the police state. We abhor the denial of free inquiry, free thought, and free expression of opinion. There are those among us who in the face of the danger would argue that we must fight fire with fire—that to achieve an end so important to our survival, abrogation of normal procedures is justified. Why, they ask, should we tolerate "fifth amendment communists"? If they are not communists, they must be perjurers, for how, if they are not communists, could they possibly incriminate themselves by testifying freely and completely?

In *The Fifth Amendment Today*, Erwin Griswold, dean of the Harvard Law School, clearly and logically presents the justification for the fifth amendment and for due process. He traces the growth and spread of these concepts from their origins in 12th-century England, shows how they were closely linked with the abolition of torture, and describes the way in which they became an essential part of the tradition of the free democratic societies of today.

By means of examples that are entirely hypothetical, but ones that the reader will immediately see are closely paralleled by real cases, it is shown how well-meaning persons completely innocent of wrongdoing, either legally or morally, may feel forced to invoke the fifth amendment to protect themselves or their equally innocent associates. Consideration is also given to the plight of the witness before an investigating committee who waives the fifth amendment privilege for himself but refuses to testify concerning others. In thus refusing to protect himself by bringing suffering upon others, whom he may have every reason to believe are innocent, such a person may find himself in strong moral position but with embarrassingly little to stand on from a strict legal point of view.

In a second chapter devoted to due process, Griswold reviews the relation of legislative investigating committees to the judicial branch of the Government and offers a series of suggestions on what a proper code of practice for such committees should be. In this he emphasizes that the responsibility for formulating and adopting such a code must rest solely and squarely upon the two houses of the Congress. Let us hope that all members of the Congress will read Griswold's little book.

As a final plea for the fifth amendment, the reader is asked to consider what it would mean to our society if this amendment were done away with. How could we then protect ourselves against those methods of the police states that could so easily undermine our whole system of free government?

The fifth amendment has had a long and honorable history. It lies at the heart of the system of justice on which our society is based. To quote Griswold:

> It is an ever-present reminder of our belief in the importance of the individual, a symbol of our highest aspirations. As such it is a clear and eloquent expression of our basic opposition to collectivism, to the unlimited power of the state. It would never be allowed by communists, and thus it may well be regarded as one of the signs which sets us off from communism.

It is a pretty comforting amendment to have around.

GEORGE W. BEADLE

Division of Biology,
California Institute of Technology

A History of Technology. vol. I, *From Early Times to Fall of Ancient Empires.* Charles Singer, E. J. Holmyard, A. R. Hall, E. Jaffe, R. H. G. Thomson, and J. M. Donaldson, Eds. Oxford Univ. Press, London–New York, 1954. iv + 827 pp. Illus. + plates. \$23.55.

This first volume of a survey of the development of technology is written on a scale never before attempted. Later volumes will deal, respectively, with Greece, Rome, and the Middle Ages (vol. II), first impact of science on technology 1500–1700 (vol. III), beginnings of the Industrial Revolution 1700–89 (vol. IV, and the 19th century (vol. V). The present volume deals with the technical arts among primitive peoples and in the most ancient empires, for example, Egyptian and Babylonian. There are 31 chapters, each written by a different author, chosen because of his eminence in the subject. The skill of the editors is manifested in the way in which the separate contributions are integrated with one another as well as in the choice of authors.

An introductory set of chronological tables enables the reader to place the materials in the book in their proper time-scale. There are general chapters on such subjects as speech and language, time-reckoning, and discovery, diffusion, and invention. Special sections deal with food-collecting, domestic activities, specializing industries (for example, domestication of animals, cultivation of plants, textiles, tools, building), utilization of metals and woodworking, and transport. The concluding section is devoted to "Preparation for science," recording and writing, measures and weights, ancient mathematics and astronomy. To make the work as useful as possible, there are bibliographies and a complete index.

Accurate, informative, and highly readable, this splendid volume presents the record of the growth of man's technical skills in the ages before the advent of science. It should be of the greatest interest to archeologists, anthropologists, engineers, historians, and scientists who wish to know more about the

growth of man's control and understanding of his environment. The volume as a whole proves that the knowledge and insight of specialists can be made available to the general reader with dignity and without loss of critical standards.

I. BERNARD COHEN

Harvard University

The Scientific Revolution, 1500–1800. The formation of the modern scientific attitude. A. R. Hall. Longmans, Green, London–New York, 1954. xvii + 390 pp. Illus. $3.50.

The period 1500–1800 saw the formation of the intellectual movement known as modern natural science. A. R. Hall, lecturer in the history of science at Cambridge, regards the thought of the previous age not as "unscientific" but simply as different, and that of the last century and a half not as different but rather as "more scientific," in the sense that the approaches of more recent scientists differ from those of 1800 chiefly in the refinement of detail. Thus he sees this period neither as an "awakening," as it used to be called, nor as a "phase," as some now would have it, but as the crucial period in the genesis of a development in the course of human events the importance of which few of the present generation are likely to minimize.

The book begins with a judicious and well-informed summary of medieval science, which exhibits, as does the entire book, an impressive familiarity with the literature on the subject. The treatment of astronomy and mechanics, in which the author has already published distinguished work, is excellent. Chemistry and biology are treated less originally but are not neglected. As befits a book described by its author as a "character study" of the scientific revolution, approximately one-third of the space is devoted to consideration of philosophy and methods. Well-selected bibliographies and appendixes are included and there is an index. I know of no more sound introduction to the history of modern science.

ROBERT MULTHAUF

Smithsonian Institution

American Men of Science. vol. I, *The Physical Sciences.* Jaques Cattell, Ed. Science Press, Lancaster, Pa.; R. R. Bowker, New York, ed. 9, 1955. 2180 pp. $20.

A new edition of *American Men of Science* is always an important event and a welcome addition to a scientist's bookshelf. As the scientific population grows, the necessity for this biographic reference work increases; since the eighth edition appeared in 1949, the number of names included has increased from 50,000 to 90,000. This increase convinced the publishers that a single volume was no longer practicable. In splitting 90,000 names into separate volumes, the publishers had to choose between making the division alphabetically and making it along subject-matter lines. They chose the latter course.

Volume 1 includes physical scientists; volume 2 (fall 1955) will include biological scientists; and volume 3 (spring 1956) will include social scientists. Users whose interests are largely confined to getting information about persons in a particular field, such as geology or mathematics, will find this arrangement convenient and will save money by buying only one volume.

There are, however, major disadvantages to the volume separation. The question of where to include biochemists and biophysicists illustrates the fact that not all scientists can be classified neatly into one of three pigeonholes. Each member of these groups was given his choice of a listing in volume 1 or volume 2. Some chose one way; some, the other. This made it necessary to list practically all names in both volumes, with a biography in one and a cross reference in the other. The cross-reference feature is also used in the inclusion of some names in the new edition with a reference to the eighth edition for detailed information. The book is therefore less handy than an alphabetic division would have been for users who are interested in all fields of science or in those fields that do not fit neatly into the current arrangement.—D.W.

Composition of Scientific Words. A manual for the methods and a lexicon of materials for the practice of logotechnics. Roland Wilbur Brown. Published by the author, U.S. National Museum, Washington 25, D.C., 1954. 882 pp. $8.

The legion of dictionaries gains recruits almost daily, but most such recruits are the stuff that privates are made of, following in well-defined paths. This "manual and lexicon" has the stuff of leadership in it, for it is not designed to be consulted passively but to enable its users to make new words. Since most of these new words will be the names of plants and animals, the book is especially rich in examples of how such names have been made. While the bulk of the book consists of the lexicon, the 54-page introduction is the essential guide to the lexicon. This introduction is not only an essay on the origins of the language and the nature of Greek and Latin but it is a readable essay on the nature of words. In it the author does not suppress his own opinions, which are often entertaining, but there are many practitioners of nomenclature who will part company with him on his recommendations about correcting the deficiencies of previous namesmiths. But this is the old controversy between those who want a name to make sense and those who regard it as simply an arbitrary convention.

The lexicon itself is a fantastic compilation. It does not attempt to be a simple dictionary, listing as many words as possible in order, but is rather a sort of thesaurus, grouping words by concept or general meaning. Under "bad" for example, are two pages of words, some synonyms, others simply uncomplimentary or unfavorable adjectives of various degrees; "bog" is followed by a similarly long list of all sorts

of sacklike containers; the entries under "bind" fill nearly three pages. This sort of thing will be of great help to the desperate systematist in search of a new name in some large genus, or to anyone else in search of a root on which to graft a new word in this day of complex molecules and disintegrating atoms. A further aid are the special sections here and there in the lexicon, such as the discussion of diminutives (under "little") and figures of speech; these are listed in the index. A typical entry gives the derivation, combining forms, and gender of the word. Pronunciation has been omitted, since this is debatable for Greek and Latin, but the author's recommendations are to be found in some sprightly passages (with amusing examples) in the introduction. It does not seem possible that a book this size should be innocent of typographic error, but after several days of dipping into it here and there, I have been unable to find one.

In short, this is the dictionary that many of us have long been waiting for, and it should be on the shelf of all who want to understand the words they use.

JOEL W. HEDGPETH
Scripps Institution of Oceanography

An Autumn Gleaning. Occasional lectures and addresses. Henry H. Dale. Interscience, New York; Pergamon Press, London, 1954. x + 225 pp. $4.25.

This is a collection of lectures and addresses delivered between 1935 and 1952 by the venerable British medical scientist, Henry Hallett Dale.

This volume presents a wealth of material on the development and nature of the intellectual, moral, and social aspects of science during the last century, particularly in medical research. It reflects the valiant struggles of man with life under the increasingly confusing impact of rapidly growing science and technology.

Typical of these essays is the Huxley memorial lecture on viruses. Dale discusses the controversial literature by Tyndall, Pasteur, Buffon, Huxley, Needham, and others on spontaneous generation and its relation to the modern researches on the generation of ultramicroscopic viruses and bacteriophage.

Extracts from his five wartime presidential addresses to the Royal Society present such problems as the relatively long-range advantages of freedom in individual research and of organization in group research; the dangers of entangling science with politics; the dangers of neglecting fundamental in favor of applied research; the importance of recognition of achievement in the more fundamental and academic ranges of science; the significance of the numerous meetings held during the war in commemoration of the tercentenary of the birth of Newton, not only in English-speaking countries but also in such out-of-the-way places as Novosibirsk: "Newton's achievement is a part of the common heritage of all peoples"; and the evils of secrecy forced upon science and government by the new conception of "total war."

Dale's other lectures include history of the discovery and use of insulin; freedom and function of science (Benjamin Franklin's question to candidates for admission to the scientific junto was: "Do you love truth for truth's sake, and will you endeavor impartially to find and receive it for yourself, and communicate it to others?"); science in education; accident and opportunism in medical research; medical research as an aim in life; Thomas Addison as a pioneer of endocrinology; the mechanism of anaphylaxis; and transmission of effects from nerve endings.

This book should be of interest to the numerous professional admirers and friends of Henry Dale as well as to philosophically minded laymen not overwhelmed by the implication of the equation $E = mc^2$.

SAMUEL BRODY
Department of Dairy Husbandry, University of Missouri

Physics and Mathematics

Concepts of Space. The history of theories of space in physics. Max Jammer. Harvard Univ. Press, Cambridge, Mass., 1954. xvi + 196 pp. $3.75.

This is a scholarly history of the concept of space, starting from antiquity and extending to contemporary times. The great scientists and religious leaders pass in rapid kaleidoscopic procession, each contributing his concept of the space which forms the universe. In early times, and even in much of Newton's philosophy, contemporary theology restricted the notions of cosmology. Gradually the experimental approach became predominant. One sees how Isaac Newton considered geometry as a phase of mechanics, but insisted that the center of mass of the sun's planetary system must be at absolute rest. Leibnitz and Huyghens argued for relative motion. The argument was finally settled by the Michelson-Morley experiment. Then Riemann became the prophet of non-Euclidean space. To test this hypothesis, Gauss hauled surveying equipment up to the tops of three high mountains, and when he did not detect any deviation from 180° for the sum of the interior angles of the triangles formed by the three mountain peaks, he concluded that space was indeed Euclidean. Lobachevski tested the hypothesis using astronomical triangles, and so began our modern attack on cosmology.

In a brilliantly written foreword, Einstein divides the basic concept of space into two viewpoints: "(a) space as positional quality of the world of material objects; (b) space as container of all material objects." One gathers that general relativity merges the two concepts into one. That is, space can be thought of as boxes but the size and shape of these boxes are modified by the presence of matter.

Max Jammer is to be congratulated on the large and interesting selections of excerpts that he has given from original documents. Most of these are given in Latin, French, or German. Although for true scholars the subtle meanings might be lost in translation, for more casual readers footnotes providing the transla-

tion would have been very helpful. For example, I missed a number of the key points because of my lack of linguistic proficiency.

I believe that the author missed a great opportunity in not stressing the interlocking of space and time. In many ways, time seems more mysterious than geometrical space. I had hoped that this book would shed some light on the Eddington Peter-Paul paradox (the time shortening journey), but this topic is not considered. As a matter of fact, the last chapter, "Concepts of space in modern science," is much too condensed. There is an obvious effort to give the names of all the people who are engaged in the philosophy of cosmology and only one sentence or equation summarizing the views of each.

Because the author cites the work of hundreds of people in the order of their appearance on the historical stage, the text seems somewhat jumpy and it is hard to follow any thread of continuity. However, the treatment is extremely thorough, especially with respect to pre-twentieth century work. The author has done an excellent job of explaining the significance of each of the different contributions. Undoubtedly this book will serve as a very valuable reference to philosophers.

JOSEPH O. HIRSCHFELDER
Department of Chemistry, University of Wisconsin

Advances in Electronics and Electron Physics. vol. VI. L. Marton, Ed. Academic Press, New York 10, 1954. xi + 538 pp. Illus. $11.80.

With the present volume the title of this excellent and authoritative series has been modified from *Advances in Electronics* to *Advances in Electronics and Electron Physics.* Since more than half of the papers are in the domain of solid-state physics, we can infer that the editor and publisher are interpreting electron physics in the pleasant and broad sense that J. J. Thomson might acknowledge.

The volume opens with a well-written review (46 pp.) by A. B. Pippard (Cambridge) of interesting recent developments in metallic conduction at high frequencies and low temperatures. An account is given of conductivity in the region of the anomalous skin effect, where the electronic mean free path exceeds the classical skin depth. Recent interpretations of infrared reflectivity measurements are discussed. The unusual position of bismuth is pointed out; the mean free path of conduction electrons in bismuth is of the order of several microns at room temperature, whereas in simple metals it is necessary to cool to liquid helium temperatures to attain comparable values.

E. Abrahams (Urbana) has provided a clear report (23 pp.) on relaxation processes in ferromagnetism—a subject that is increasing in importance with the recent practical interest in the use of ferromagnetic ferrites at high radio and microwave frequencies. It is clear that we do not yet have a complete understanding of relaxation mechanisms, but a number of suggestive calculations have been made by Akhieser, Abrahams, Keffer, Van Vleck, Luttinger, and others. The review was prepared too long ago to include accounts of recent relevant work by Galt, Uehling, Stevens, Rado, and Bloembergen.

The physical properties of ferrites are reviewed by J. Smit and H. P. J. Wijn (Eindhoven). This 63-page paper contains only an abridged account of the Néel theory and of the pertinent neutron diffraction experiments. The discussion of magnetization curves and losses under various conditions is particularly comprehensive and is the outstanding feature of the article. The fascinating and unique applications of ferrites at microwave frequencies are touched on in passing.

H. F. Ivey (Westinghouse, Bloomfield) has written a 120-page paper on space-charge limited currents, treating in detail and with great care the general theory of the principal geometric arrangements. This review should be of considerable use to designers and to research workers who are concerned with electron-beam devices.

W. M. Webster (R.C.A., Princeton) has made an interesting comparison of the behavior and properties of analogous semiconductor and gaseous electronics devices. The article is quite condensed but will be of value to anyone with a fair background in both fields. An important section is devoted to a discussion of the theoretical limitations of semiconductor devices.

M. E. Haine (Associated Electrical Industries, Aldermaston) reviews in 74 pages the current state of our knowledge of the electron microscope. He treats resolving power, image contrast, magnetic and electrostatic lenses, spherical corrections, astigmatism, the projector lens, the electron gun, the object stage, alignment, and a number of other practical design features. The article is intended for designers and for the users of the more than 1000 electron microscopes now in operation.

R. G. E. Hutter (Sylvania, Bayside) has contributed a 90-page review of the rapidly developing field of traveling-wave tubes, intended to bring out common, as well as distinctive, features of the various types of traveling-wave tubes. The general theory is developed at some length, following the method of J. R. Pierce. The broad band width of some of these tubes as amplifiers and as oscillators is of unusual practical importance. The recent backward-wave tubes are discussed.

The volume closes with a 55-page review of paramagnetism by J. Van den Handel (Leiden). This article is far too compact to be of real value. No fair impression is given of the tremendous postwar activity in the field.

The general educational usefulness of most of the articles would be enhanced if the introductory sections were enlarged considerably and written in elementary physical and pictorial terms at the level of a beginning graduate student in physics and engineering. The editor has done a splendid job in gathering together distinguished and interesting contributors; he should now ask them to throw more scraps to the

man in the street. The cumulative length of all the introductions to the eight review articles in this volume is about 20 pages; the volume contains more than 500 pages. The fruits of this fine effort would be enjoyed more widely if there were at least 100 pages of introductions.

C. KITTEL

Departments of Physics,
University of California, Berkeley

High-Energy Accelerators. M. Stanley Livingston. Interscience Tracts on Physics and Astronomy, No. 2. R. E. Marshak, Ed. Interscience, New York–London, 1954. viii + 157 pp Illus. $3.25.

This is a very useful book introducing the reader to the various types of electronuclear machines. The circular machines are discussed in some detail and the mathematics of the more elementary problems of orbit dynamics are given. Otherwise, the text is principally descriptive. The book contains an initial survey chapter indicating the various energy thresholds successively passed in the advance of the accelerator art. This section contains a very helpful outline of the more prevalent meson and hyperon reactions. The final section contains an illuminating graphical representation of the energies attained by the various machines in relation to the time.

High-Energy Accelerators reflects, of course, the author's own interests and experience. For this reason, the degree of detail devoted to the various instruments is somewhat subjective.

The book is very well written and fills a very real need. It will be a great help to the reader in evaluating the comparative properties of the various devices.

WOLFGANG K. H. PANOFSKY

High-Energy Physics Laboratory,
Stanford University

Existence Theorems for Ordinary Differential Equations. Francis J. Murray and Kenneth S. Miller. New York Univ. Press, New York, 1954. (Distrib. by Interscience, New York.) x + 154 pp. Illus. $5.

In this book the authors have collected several of the important existence theorems for differential equations. Beyond the obvious usefulness of having such theorems discussed in a logical sequence, the authors' principal aim is to show the role that such theorems play in computational aspects of differential equations. The development of high-speed computers and their use in numerical solutions of differential equations have proceeded at a rapid pace. Existence theorems are essential if one is to understand in what sense such solutions are reliable. From this point of view the authors have, in my opinion, done a creditable job.

The book begins with the existence theorems of Peano for the differential equation $\frac{dy}{dx} = f(x,y)$, continues to a system of equations and, hence, to implicit equations. These first theorems are strictly existence theorems without uniqueness. The methods are the usual ones of approximation by line segments, equicontinuous functions, and the Ascolis theorem. They then proceed to equations with more restrictive conditions (Lipschitz condition) and show, as a result, how one obtains uniqueness. Solutions of the important equations $\frac{dy}{dx} = f(x,y,\lambda)$, or systems of such equations containing a parameter, are discussed as a function of λ and also as a function of the initial conditions. In particular, theorems are given concerning continuity and differentiability of solutions of such equations with respect to the parameter or initial conditions. The book concludes with theorems in the large for linear equations and finally equations with constant coefficients.

The theorems discussed in the book are all well known to those who have studied existence theorems of Peano, Picard, Bliss, and others. However, the authors have rendered a service to those not so familiar with the field in bringing these theorems to their attention. The book is well written and can be read and understood by one with a reasonable foundation in the classical real variable theory.

C. P. WELLS

Department of Mathematics, Michigan State College

Sonics. Techniques for the use of sound and ultrasound in engineering and science. Theodor F. Hueter and Richard H. Bolt. Wiley, New York; Chapman & Hall, London, 1955. xi + 456 pp. Illus. $10.

The aim of this book is to bring together in a unified presentation the basic principles and techniques of a young technology, here appropriately designated as *sonics*, now just coming of age. This new field of applied science deals with mechanical vibratory energy (whether of high or low frequency) as it is now being applied to such varied problems as oil-well drilling, medical diagnosis, gas analysis, and metals testing. The authors recognize that of the many persons who are interested in this field only a few have had advanced training in acoustics, and have therefore aimed (successfully) at making most of the material understandable to anyone with college training or the equivalent in any branch of science or engineering.

A wealth of pertinent and very up-to-date technical material on sonics has been assembled and skillfully integrated, much of it from technical reports and other sources not conveniently available. Four of the eight chapters are packed with developments of formulas and principles from well-established theory of sound propagation and transducer design; another chapter reviews knowledge on physical mechanisms for effects caused by sound. Two extensive chapters describe devices, techniques, and design principles found to be effective in typical processing or testing problems; an appendix contains an informative review of the contributions of acoustical relaxation studies to

basic physics. Many graphs of important formulas and tables of pertinent data are provided throughout the text, making it a valuable and reasonably self-contained sonics handbook.

This attractive, well-organized book, by eminent authorities in its subject, is highly recommended to anyone seriously interested in physical, chemical, or biological applications of vibrations, sound, or ultrasound.

WESLEY L. NYBORG
Physics Department, Brown University

Higher Algebra. vols. I and II. Helmut Hasse. Trans. by Theodore J. Benac. Frederick Ungar, New York, Eng. transl. of rev. Ger. ed. 3, 1954. 336 pp. $6.50.

Exercises to Higher Algebra. Helmut Hasse and Walter Klobe. Trans. by Theodore J. Benac. Frederick Ungar, New York, Eng. transl. of rev. Ger. ed. 2, 1954. 212 pp. $4.

The English-reading mathematical fraternity is fortunate in now having available an excellent translation of Helmut Hasse's famous *Höhere Algebra* and its accompanying volume of exercises by Hasse and Walter Klobe. Both the translator and the publisher are to be congratulated on a valuable job well done. The two parts of *Higher Algebra* are bound together; the exercises are in a separate matching volume. Typography and format are very good. The translation, carefully made from the latest German editions, contains little of the stilted phraseology often found in English translations of German works.

The characterizing feature of algebra has undergone a transition since the days of Cardano and Tartaglia. In the early days algebra was generally understood as the theory for solving equations built up by means of a finite number of the four elementary operations performed upon real (or complex) numbers and unknowns. The material of this phase is neatly packeted now in almost any college textbook on algebra and the theory of equations. Next, in an effort to gain deeper insight into the operations involved, a greater generality was achieved by ignoring the intuitive significance of the numbers involved and by considering these numbers as mere symbols obeying certain formal rules. Thus, in this middle period of algebra, such concepts as ring, field, integral domain, and group came into being. Questions of solvability dependent upon a ground field were considered, and the Galois theory was developed. In modern times the center of interest has shifted from the solution of equations to a concentrated study and generalization of these concepts, and today we have college textbooks of modern, or abstract, algebra. Hasse's work adopts the point of view of the middle period. It thus appears as a blend of the classical and modern viewpoints. It considers as the basic problem of algebra the following: Given a field K and a set of elements $f_1, \ldots, f_m$ of the integral domain $K[x_1, \ldots, x_n]$, to develop methods for obtaining all solutions of the system of equations $f_i(x_1, \ldots, x_n) = 0$, $(i = 1, \ldots, m)$. Since the complete theory for the solution of this problem is too voluminous, Hasse restricts himself in volume 1 to linear systems of equations in K and in volume 2 to single algebraic equations of rth degree in K. The necessary field and group theory is developed as needed.

A citation of the chapter titles shows the specific content of Hasse's work: Vol. 1, "Rings, fields, integral domains," "Groups," "Linear algebra without determinants," and "Linear algebra with determinants"; for Vol. 2, "The left sides of algebraic equations," "The roots of algebraic equations," "The fields of the roots of algebraic equations," "The structure of the root fields of algebraic equations," and "Solvability of algebraic equations by radicals."

The accompanying exercises are valuable and are classified under the successive topics of the text proper, and contain, in most cases, helpful comments and hints. These exercises constitute one of the finest available collections in this field. The text contains no exercises, but is illuminated by numerous examples. An excellent advanced undergraduate or beginning graduate course can be based upon *Higher Algebra* and its accompanying collection of exercises.

Finally, I would like to mention the pleasure one can receive by reading this work. It is written by a master expositor in elegant style, and with meticulous care, and it contains many, many sidelights and mathematical niceties.

HOWARD EVES
Mathematics Department, University of Maine

Macroscopic Theory of Superfluid Helium. vol. II of *Superfluids.* The late Fritz London. Wiley, New York; Chapman & Hall, London, 1954. xvi + 217 pp. Illus. $8.

The exciting physical properties of liquid helium comprised a subject of consuming interest to Fritz London and his original writings on the theoretical aspects are known throughout the world of science. This book is outstanding and scientists everywhere will always be grateful to him for the thorough, well-written, summation of the quantum theoretical treatment of liquid helium. The book joins the first volume, *Superconductivity,* as a lasting monument to him. It has become an indispensable textbook for graduate-school courses and seminars concerned with the subject of low temperature physics. The volume on superfluid liquid helium has already demonstrated its enormous value to scientists—one finds it quoted so often in the original writings in scientific journals. Nothing I can say in further praise of London's book can equal the steady tribute paid to it by research scientists in their articles.

The first chapter gives a 13-page review of the remarkable properties of liquid helium. This is followed by a chapter that gives theoretical conclusions as to why liquid helium must remain a liquid all the way down to the absolute zero of temperature. In writing

the third chapter about the two-fluid concept and the application of Bose-Einstein statistics to liquid helium, London must often have recalled the happy days in Paris when these ideas came bursting out of him, and of his stimulating contacts at that time with L. Tisza and his many other colleagues. It is in this third chapter, too, that one finds the discussion of the fountain effect, the viscosity studies, and the thermo-mechanical experiments which were all a part of the prewar era.

The fourth chapter is indeed a great service to all of us because London has done a very capable presentation of Landau's theory about liquid helium, that is, the concept of rotons and phonons. Throughout all this he has enriched the theoretical treatment with well-displayed graphs of the experimental work. The last two chapters are devoted to special topics under the heading: "Two-fluid thermo*hydro*dynamics," and "The helium isotope He^3." It is here that one finds such things as the interpretation of the experiments with the Rayleigh disk using thermal waves (or second sound). The author had completed the manuscript for this book in the late winter of 1952, and I remember celebrating with him his revision of the galley proofs and their return to the publishers in late June of 1953 —again in Paris! During the month of December 1953, London attended an International Conference on Low Temperature Physics and served as chairman of the session devoted to R. P. Feynman's recent theory of liquid helium. Probably had his health not failed, he would have revised the page proofs to include at least a footnote of what he knew to be important and controversial. It would have been typical of London, for he was a thorough and great scholar. This book is a memorial to his genius and has been fittingly dedicated to his wife, Edith London.

CHARLES F. SQUIRE
Department of Physics, Rice Institute

Chemistry and Biochemistry

Organic Chemistry. Lewis F. Hatch. McGraw-Hill, New York–London, 1955. vii + 324 pp. Illus. $4.50.

As a palliative for the onus of organic chemistry on "students majoring in home economics . . . agriculture . . . or . . . other fields who do not require a full year . . ." this textbook serves adequately in that it contains the basic elements of the subject, presented appetizingly in a simple readable manner, embellished by numerous photographs and diagrams, and sprinkled with homely bits of humor. The abbreviation necessary in a textbook designed for use in a one-semester or -quarter survey course is accomplished, probably wisely, through broad coverage at the expense of the attention to detail usual for the full-year treatment.

In the first chapter Hatch introduces the reader, by way of the hydrocarbons, to positional isomerism and the nomenclature of aliphatic and aromatic substances, and in subsequent chapters no formal division is made between members of the two broad groups. Of the remaining 19 chapters, 15 are devoted to the time-honored topics, including the simple classes of organic compounds as well as dyes, proteins, fats, waxes, and carbohydrates. The remaining four chapters, entitled "Enzymes, vitamins, hormones and antibiotics," "Medicinals and pharmaceuticals," "Polymers," and "Organic chemistry and agriculture," are noteworthy since the material included, timely and up to date, should be of special interest to the type of student for whom the textbook is designed.

In general, the number of reactions covered is adequate; the consideration given to the utilization and practical importance of organic materials is ample; and structural formulas, including those of, for example, terramycin, Aureomycin, streptomycin, and chloromycetin, are abundant. The more theoretical aspects of organic chemistry are, however, treated cursorily. For example, two topics that are likely to give trouble to most beginning students, the concepts of resonance and optical isomerism, are covered in two and six pages, respectively. The mechanisms of organic reactions are discussed on occasion.

On perusing the book, I uncovered no typographic mistakes; factual errors are at a minimum and are not of a serious nature—certainly a structure for α-pinene that violates Bredt's rule will not seriously hamper the desired development of the average student in the course.

The book, sturdily bound with an attractive cover, features with each chapter a short list of collateral reading topics and a group of about 10 to 20 problems, without answers, of a degree of difficulty consistent with the probable needs of the course.

EUGENE E. VAN TAMELEN
Department of Chemistry, University of Wisconsin

Electrometric pH Determinations. Theory and practice. Roger G. Bates. Wiley, New York; Chapman & Hall, London, 1954. xiii + 331 pp. Illus. $7.50.

Everyone measures *p*H, but relatively few appreciate what it is not. Since the original proposal of Sörensen, a number of suggestions have been made in the attempt to establish practical scales that would experimentally satisfy the ideal definition "negative logarithm of hydrogen ion activity." However, exact agreement has never been achieved because an exact measurement of the true activity of hydrogen ion, or indeed of any single ion species, has not been accomplished. The fundamental difficulty is the existence in any practicable cell of a liquid-junction potential whose magnitude is somewhat dependent on the composition of the test solution, and which is not amenable to rigorous thermodynamic interpretation. Consequently, practical scales of *p*H numbers necessarily involve nonthermodynamic approximations that render them distinctly empirical and only in approximate accord with the ideal definition. No one has a more keen appreciation of this situation than Roger Bates, and his discussion of the definitions of *p*H

scales is the most complete and lucid that I have seen.

Indeed, "complete and lucid" applies throughout this excellent book. All aspects of the definition, measurement, and interpretation of pH are treated in an admirably balanced fashion. If you seek information on theoretical fundamentals, you will find it in the first three chapters. If your need is advice on pH standards and buffer solutions for calibrating a pH meter, chapters four and five will provide it. If you have measured an apparent pH number in a nonaqueous test solution and are faced with the question "What does it mean?," you should study the fifth chapter. Perhaps you need to know how to construct and use a hydrogen electrode or a calomel electrode, how to care for a glass electrode, under what conditions you can use a quinhydrone or antimony electrode, how to check the functioning of a pH meter and recognize probable cause of malfunctioning, and what simple servicing steps are likely to remedy the difficulty, or the operational principle and equipment used in automatic pH control. The book is a rich source of specific, satisfying answers to practical questions of this kind.

Perhaps there are typographical errors, but I have not searched for them. Neither have I examined the binding nor inquired the book's price. Reference books of this quality are the cheapest tools we use. Whatever the price it will be insufficient to provide more than a token recompense to the author for the invaluable contribution he has made.

JAMES J. LINGANE

Department of Chemistry, Harvard University

Glutathione. A symposium. S. Colowick, A. Lazarow, E. Racker, D. R. Schwarz, E. Stadtman and H. Waelsch, Eds. Academic Press, New York, 1954. x + 341 pp. Illus. $7.50.

This book contains the papers and some of the discussion presented at a symposium held at Ridgefield, Conn. in Nov. 1953. Its purpose is to collect various aspects of knowledge about glutathione that otherwise would remain widely scattered in the literature. It is not convenient to catalog the 29 papers nor to list all 57 contributors, but the scope and significance of the conference can be briefly indicated. The book is divided into four sections: "Properties and organic chemistry," "Methods of detection and assay," "Biochemical mechanisms," and "Physiological and clinical aspects." The first section contains a valuable discussion of the physical and organic chemistry of mercaptans and disulfides as well as several papers devoted to specific aspects of the chemistry of glutathione. The second section is devoted to classical methods of glutathione assay, to paper chromatography, and to histochemical studies on organic sulfur compounds.

The section on biochemical mechanisms is longest and most diverse in the variety of topics considered. It includes papers on the enzymatic oxidation and reduction of glutathione, the biosynthesis of glutathione, the role of glutathione in transpeptidation, and a discussion of γ-glutamyl transfer reactions. The role of glutathione as a coenzyme is surveyed, as well as the broader topic of the transfer of acyl groups by sulfhydryl compounds such as lipoic acid and coenzyme A. Also included in this section is an extensive discussion of the role of sulfhydryl groups in mitosis. The last section includes discussions of the relation of glutathione to hormone action, to radiation injury, and to organic and mental diseases. The book closes with good author and subject indexes—a valuable feature that is too often lacking in symposium volumes. Many of the papers have extensive bibliographies so that the book is an excellent key to the literature on glutathione.

In addition to summarizing and documenting available knowledge of glutathione, this book contains interesting comparative information on lipoic acid, coenzyme A, and sulfhydryl-containing proteins, and it may be expected to be of general interest to those working in the fields of biochemistry, physiology, and medical research. The principal faults in *Glutathione* are evidences of imperfect copy editing and proofreading and a lack of editing of the discussions. One finds questions that are not answered, answers that lack antecedent questions, and obscure and ungrammatical sentences. The latter must be blamed on the authors, because the discussion comments were transcribed from recordings and returned to the authors for revision.

In these days of much advertised conflicts between scientists and the various branches of government, it is very pleasant to find one act of the government that scientists can unanimously acknowledge with gratitude. A major factor in the success of this symposium was the financial assistance granted by the National Science Foundation and the Office of Naval Research. This is of value, not only to the participants in the symposium but to all scientists who may have occasion to read the report of the symposium proceedings.

MARK H. ADAMS

Department of Microbiology,
New York University College of Medicine

Micro and Semimicro Methods. Nicholas D. Cheronis. vol. VI of *Technique of Organic Chemistry*; Arnold Weissberger, Ed. Interscience, New York–London, 1954. xxiii + 628 pp. Illus. $12.

In this sixth volume of the comprehensive treatise *Technique of Organic Chemistry*, an attempt has been made to give detailed coverage to micro and semimicro methods. How thoroughly this has been done is reflected in the fact that no less than 229 pages are devoted to intimate discussions of laboratory manipulations on the micro and semimicro scale. These cover such topics as crystallization, distillation, sublimation and extraction, measurement of volume, and the determination of other physical constants such as melt-

ing points, boiling points, densities, refractive indices, and molecular weights. These chapters abound in descriptions and diagrams of apparatus, and the intricacies of the latter are so minutely discussed that it is difficult to see how anyone, graduate or undergraduate, could have trouble in using this book in the laboratory as a sort of self-teaching manual. This feature is not accidental but has been introduced by the author who states in his preface that "the number of organic chemists who have practical experience with these procedures is still small."

The second part of the book consists of a systematic collection of preparative reactions. Here again one finds detailed treatment of various micro and semimicro methods for carrying out such basic processes as reduction, oxidation, halogenation, acylation, esterification and hydrolysis, nitration and sulfonation, amination and diazotization, dehydration, cyclization and condensation. Also included is a short chapter on the use of organometallic compounds, and a longer and interesting chapter on microsynthesis with tracer elements. The material in the latter chapter should be of great practical importance to anyone about to embark upon tracer work, for it contains much sage advice, together with cautions and precautions.

Part three deals very thoroughly with analytic procedures and their underlying reactions. Here are more analytic tests and characterization reactions than are usually studied in comprehensive courses on qualitative analysis. This section also includes a short chapter on the quantitative estimation of functional groups.

Some readers may feel that too much space is devoted to the more elementary features of manipulation and analysis. In my opinion, the book would have benefited by a section dealing with the handling of oxygen-sensitive compounds, such as the carotenoids, details concerning which must still be sought in the original literature. All who read the book, however, will have to agree that the author has done an exceedingly thorough job with the areas covered. It appears remarkably free from error, is well written, and should be of great service to both student and teacher, as well as to the research worker.

EVANS B. REID

Department of Chemistry,
Colby College

Principles of Biochemistry. Abraham White, Philip Handler, Emil L. Smith, and DeWitt Stetten, Jr. McGraw-Hill, New York–London, 1954. xiii + 1117 pp. Illus. $15.

The four authors of this textbook are experienced teachers and investigators in biochemistry. Through a process of repeated writing, consultation, and revision they have produced a textbook for first-year medical students, no section of which is said to be the work of a single author. There is evident a considerable integration and correlation, not only within the subject field but with other aspects of the student's first year and with his future studies. The authority of the book is unquestionable. Any disagreement with the presentation will generally be on the basis of emphasis and interpretation.

There are seven parts comprising 50 chapters. The parts are successively entitled "Chemical composition of cells," "Catalysis," "Metabolism," "Body fluids," "Biochemistry of specialized tissues," "Biochemistry of the endocrine glands," and "Nutrition." In spite of the title a large amount of descriptive material is necessarily included and the more difficult abstract and mathematical principles of chemistry applicable to biology are not emphasized. An interesting experiment is the treatment of digestion of each of the great classes of foodstuffs in the chapters devoted to their metabolism, with additional material concerned with digestion appearing under body fluids and elsewhere.

The material includes presentation of as much recent experimental and theoretical work as a textbook can possibly have. References to monographs and reviews are a feature and some individual experimental papers are included. Some working hypotheses, because of their presentation in this book will be regarded by inexperienced readers as well established theories even though the authors have properly qualified their own acceptance. Most teachers in biochemistry find it necessary to take this risk because a connected and reasonable story seems essential in a field where gaps and lacks are many in spite of or perhaps because of the rapid growth of the science.

This textbook will be widely adopted and will have great influence in the medical field. It will fulfill its projected function and will require revision in a few years. This is the fate of nearly all textbooks in biochemistry. At this time it should be seriously considered for use by all who teach biochemistry to medical students.

MILTON LEVY

Department of Biochemistry,
New York University College of Medicine

Physical Measurements in Gas Dynamics and Combustion. vol. IX of *High Speed Aerodynamics and Jet Propulsion*. Part 1, R. W. Ladenburg; Part 2, B. Lewis, R. N. Pease, and H. S. Taylor, Eds. Princeton Univ. Press, Princeton, N. J., 1954. xvi + 578 pp. Illus. + plates. $12.50.

Rapid advances made during the last decade in the fields of gas turbines and of high-speed flight have been largely based on the development of experimental methods for the study of gas dynamics and of combustion. The present volume, a critical and comprehensive review of these methods, is a most welcome addition to the literature on the subject, particularly since the original contributions are scattered through many technical journals not always available in the same library.

The first part of the volume (340 pp.), under the editorship of the late R. W. Ladenburg, deals with

the measurements in gas dynamics. The second, under the editorship of B. Lewis, R. N. Pease, and H. S. Taylor covers experimental techniques in combustion. Chapter headings give a good idea of the scope of this work. The first part comprises: "Density measurements," "Pressure measurements," "Velocity measurements," "Temperature measurements," "Shock front measurements by light reflectivity," "Turbulence measurements," "Condensation study by absorption or scattering of light," and "Analogue methods." The second part consists of chapters on: "Measurement of flame temperature, pressure and velocity," "Flame photography," "Measurement of burning velocity," "Mass spectroscopy," "Spectroscopy of combustion," and "Analysis of the combustion wave by pressure effects and spectroscopy." These chapters are by leading investigators in the respective fields: A. B. Arons, J. W. Beams, D. Bershader, W. Bleakney, F. P. Bundy, W. M. Cady, P. M. Chambré, G. H. Dieke, G. R. Eber, E. F. Fiock, J. A. Hipple, D. F. Hornig, L. S. G. Kovasznay, R. Ladenburg, L. Malavard, W. T. Reid, S. A. Shaaf, A. H. Shapiro, H. M. Strong, N. Thomas, F. J. Weyl, and E. M. Winkler. The writing is authoritative; the difficulties encountered in making various measurements and the devices by which these difficulties have been overcome or reduced are well presented. A good balance is maintained between the discussions of the principles of the methods used and the descriptions of experimental details. Numerous drawings and halftone plates make it easy to follow presentations. The articles are well referenced throughout and while the reference lists make no claim to exhaustiveness, they seem to have been well chosen and to be entirely adequate.

As is probably unavoidable in a cooperative undertaking of this nature, some duplication and lack of uniformity are noticeable occasionally. Thus the schlieren method and the Mach-Zender interferometer are discussed in both parts of the volume, the second presentation adding little that is not covered by the first. Some comparatively simple techniques are given more space than really necessary although this space could have been advantageously used to describe the more sophisticated methods in greater detail. The reviewer questions a few of the statements made, for instance some of those about active nitrogen (pp. 79–81) or the remark (p. 345) that sound velocity measurements in flames give translational temperature regardless of equilibrium with other degrees of freedom. Such weaknesses, however, are few in number and are not important, since on the whole the volume is extremely well written and gives the reader a clear view of the present status of experimental techniques, as well as preparing him for the study of original literature.

Legibly and attractively printed, this volume should be required reading for all those preparing to undertake research in the important area of physical sciences covered by this series.

G. B. KISTIAKOWSKY
Department of Chemistry, Harvard University

Biological Sciences

Genetic Homeostasis. I. Michael Lerner. Wiley, New York; Oliver & Boyd, London, 1954. vii + 134 pp. Illus. + plates. $3.25.

In this small volume, the author has undertaken to demonstrate that Mendelian populations have self-regulatory properties, that this genetic homeostasis is related to the better-known developmental homeostasis, and that heterozygosity furnishes a common basis for both phenomena. In developing this thesis, a number of seemingly unrelated lines of evidence are cleverly selected and woven into an extremely convincing argument. The result is probably the most important recent contribution to the literature of population genetics.

Individuals of all species—perhaps to a greater extent among those that are cross-fertilizing—possess regulatory mechanisms that buffer developmental processes against capricious environmental fluctuations. Lerner, through a study of the data bearing on the relation of genotype to environmentally caused phenotypic variation, concludes that heterozygous individuals of a cross-fertilizing species have buffering capacities superior to those of homozygous individuals and, hence, possess "normal" phenotypes more frequently than do the latter. The consequences of this simple hypothesis for populations are far reaching: on the average, heterozygous individuals are favored by selection. Selection, not simple mutation pressure, is primarily responsible for maintaining the genetic variability within populations; the greater the variability, the greater the proportion of heterozygous individuals, and the greater the average fitness of the population. A program of selection, insofar as its aims are met by homozygosity, may be brought to a halt through an unsuspected counterselection for heterozygosity long before the genetic variability of the selected population is exhausted. Responses by populations to novel demands of natural selection are more rapid than they are usually assumed to be under a "homozygous individual" model. In spite of this ability to make rapid responses, the population preserves the ability to revert to its original state (or its equivalent) if the novel demands prove short lived.

As experimental evidence accumulates, details of the arguments presented in this essay will doubtlessly undergo modification. It may be, for instance, that the term *heterozygous* has been used in an operational rather than in a definitive sense. The rather long discussion concerning the pleiotropic effects of genes may eventually appear unduly cautious. Nevertheless, the main argument represents a refreshing approach to problems facing students of both natural and artificial selection; these would do well to take seriously the admonition of the epigraph: "Read not to contradict nor to believe but to weigh and consider."

BRUCE WALLACE
Biological Laboratory, Cold Spring Harbor, New York

The Physiology of Insect Metamorphosis. **V. B. Wigglesworth. Monographs in Experimental Biology, 1. Cambridge Univ. Press, New York, 1954. viii + 152 pp. Illus. + plates. $2.50.**

An event of immediate and enduring significance is the publication of this authoritative and lucid analysis of the physiology of insect growth and metamorphosis. The identity of the author as V. B. Wigglesworth will suffice to recommend it to biologists. However, scientists in related fields and biochemists in particular will find an evening with the volume a fascinating experience.

Wigglesworth constructs his analysis of metamorphosis around the substantial core of his own studies of the hemipteran *Rhodnius prolixus*. In addition, 386 papers are cited and woven into the discussion. By virtue of clear thinking and crisp writing the author has been able to encompass the analysis into 152 pages. There are five chapters, four pages of halftones, 45 text figures, a comprehensive bibliography, and a subject index.

The five chapters consider the origin and evolution of metamorphosis, the histological changes during moulting and metamorphosis, the physiology of growth and moulting, the physiology of metamorphosis, and differentiation and polymorphism.

Heretofore the most up-to-date summaries of the physiology of insect metamorphosis have been Pflugfelder's *Entwicklungsphysiologie der Insekten* (Leipzig, 1952, 332 pp.), and Bodenstein's chapters in Roeder's *Insect Physiology* (Wiley, N.Y., 1953, pp. 874–931). The present volume may serve as a companion piece to the Snodgrass monograph on the morphology of *Insect Metamorphosis* (Smithsonian Inst. Misc. Collections 122, No. 9) which also appeared this last year. Taken together, these two excellent volumes afford a comprehensive account of metamorphosis by the foremost authorities on insect physiology and morphology.

CARROLL M. WILLIAMS
Biological Laboratories, Harvard University

Contributions to Embryology. **vol. 35, Nos. 231–241. Carnegie Institution of Washington, Washington 5, 1954. 237 pp. Illus. + plates. Paper, $12; cloth, $13.**

This, the most recent volume of the long series of *Contributions,* fully lives up to the established reputation of its predecessors.

Its 11 monographs cover a wide variety of allied subjects. Their titles and authorship follow: 231, "Early abnormal embryos of the rhesus monkey," George W. Corner and George W. Bartelmez; 232, "Development of the baboon," Christine Gilbert and Chester H. Heuser; 233, "Formation of the neural crest," G. W. Bartelmez and Mary P. Blout; 234, "Regional circulation times in the lamb," S. R. M. Reynolds *et al.*; 235, "Androgen-induced pseudohermaphroditism in the monkey," L. J. Wells and G. van Wagenen; 236, "Development of the human diaphragm and pleural sacs," L. J. Wells; 237, "Architecture of human umbilical cord tissues," Anna W. Chacko and S. R. M. Reynolds; 238, "Venous drainage of the placenta of the rhesus monkey," Elizabeth M. Ramsey; 239, "Early development of the human nephros," Theodore W. Torrey; 240, "Preimplantation stages of the human ovum," Arthur T. Hertig *et al.*; and 241, "Erythrocyte-forming areas in chick blastoderm," George W. Settle.

Individual comment on these studies is obviously impossible in so brief a review as this. However, contribution 236 must be cited as a valiant effort of author and illustrator to surmount the inherent difficulties present in the demonstration of their subject. Only those who glory in overcoming great difficulties resort to such arduous efforts as that shown in this study.

The report of Hertig *et al.* is a fitting climax to the remarkable series of very young human embryos previously reported by this author in collaboration with Rock, Heuser, and others.

Torrey's contribution (No. 239), in the light of his interpretation of the nephric system in general, should perhaps be emphasized.

Finally, the excellence of the illustrations, especially the superb work of J. F. Didusch, must be noted.

CARL L. DAVIS
Department of Anatomy,
University of Maryland Medical School

***The Genetics of* Paramecium aurelia.** **G. H. Beale. Monographs in Experimental Biology, 2. Cambridge Univ. Press, New York 22, 1954. xi + 179 pp. Illus. $2.50.**

Modern genetics may be arbitrarily divided into three areas, each concerned with heredity and variation but focusing attention respectively upon the population, the individual, and the cell. In addition to sharing certain common concerns, each of these areas has its own special techniques and specific problems. Perhaps the central problem in "cellular heredity" is the solution of a riddle—how do cells of identical genetic constitution develop and maintain different hereditary traits? Although approaching a problem of general biological interest, for technical reasons much of the work in cellular heredity has been undertaken with unicellular organisms and particularly with *Paramecium aurelia*.

Investigations on this organism have been directed primarily toward an analysis of three cellular properties: the serotypes, the mating types, and the killer traits. Cells with the same genetic constitution may develop persistent differences in regard to each of these characteristics. Interestingly enough, the factors involved in initiating and maintaining these differences interact in each case in what appears to be a different pattern. Each of these patterns involves nuclear, cytoplasmic, and environmental participation and any or all may be of general biological significance.

Because of the extensive research completed on *Paramecium* since Sonneborn's review in 1947, this book by Beale is especially welcome. Both the older work and the recent one are clearly and authoritatively presented. The book will be of interest not only to geneticists, but to all concerned with the biology of the cell.

DAVID L. NANNEY

Zoology Department, University of Michigan

Recent Developments in Cell Physiology. Proceedings of the 7th symposium of the Colston Research Society held in the University of Bristol, March 29–April 1, 1954. J. A. Kitching, Ed. Academic Press, New York; Butterworths, London, 1954. x + 206 pp. Illus. $6.50.

If we no longer refer to a drinking party when we speak of a "symposium," it does not follow that a symposium must be a large convocation of the well-known specialists in a field for the purpose of reviewing their well-known work. The latter has become the common case, and when the results are published they may add to the growing archives of digested research, but seldom serve to dispel the impression that ideas are a scarce commodity. This volume is exceptional. It does not by any means cover the major fields of cell physiology. Only occasionally does it serve to bring the reader up to date on problems that have been dignified by inclusion in textbooks. The remarkable feature of it is that it is interesting.

The symposium reveals the healthiest feature of current research on the cell: its growth freedom from both vertical and horizontal restrictions. Vertically, we find problems of diffusion theory and biosynthesis of small molecules discussed along with problems of embryo differentiation and the control of cell division. Horizontally, the venerable problems of cell permeability consort with questions of hormone action; current theories of gene action are brought to bear on the regeneration of flatworms, and the seemingly specialized problem of suction in Suctoria comes into line with current work on the physics of cell surfaces.

A fair number of the papers included are not research reports at all, but expressions of ideas. Indeed, a selection of some of the ideas presented may serve to describe the state of fermentation that makes the life of the cell physiologist so interesting today. *Danielli*: molecules may enter the cell not only by simple diffusion or by active transport mechanisms but also by *facilitated* diffusion, in which molecules move without the performance of osmotic work at rates other than those predicted. *Koch*: cholinesterase, thus far associated mainly with nerve conduction, is shown to be a part of the mechanism of active transport of salts across certain membranes. *Ussing*: active transfer of water may occur across animal membranes and the pumps may be under hormonal control. *Yemm*: protein synthesis in plants may involve peptide intermediates and the action of genetic templates may be at an "assembly" stage rather than at the stage of peptide bond synthesis. *Hoff-Jørgenson*: DNA may be stored in the cytoplasm of ova, and DNA synthesis during development may not begin until the stores are used up. *Brønsted*: polarity and bilaterality in regeneration (*Planaria*) may be analyzed in terms of time-gradation of gene activity. *Zeuthen and Scherbaum, Maaløe and Lark*: cell division may be synchronized by subjecting cell populations to temperature cycles. (The discussions of the theory of this important effect are of great interest.) *Swann*: the control of cell division in a nongrowing system (an ovum, for example) may be treated as a problem of the distribution of the cell's energy supply to various functions.

In addition, the volume includes papers by Keynes (on the correlation of ionic movements with nerve function), Klenow (on the biosynthesis of pentoses), Brachet (on the nuclear control of enzymatic activities), Waddington (a theoretical treatment of cell differentiation, largely in terms of the plasmagene concept), Westergaard and Hirsch (control of differentiation in *Neurospora*), and a very interesting account by Kitching of his work on suction in Suctoria.

The reader of this volume will not conclude that present-day cell physiology is moving in strong currents, and this is a healthy sign. If there is any trend, it is toward increasing attention to the problems of growth, reproduction, development, and heredity. No longer is cell physiology merely the application, for its own sake, of physics and chemistry to the static abstraction of a cell. The cell is coming to life, and nothing that it does seems to be beyond experimental attack.

DANIEL MAZIA

Department of Zoology,
University of California, Berkeley

Insects of Micronesia. vol. 1, ***Introduction.*** J. Linsley Gressitt. Bernice P. Bishop Museum, Honolulu 17, 1954. viii + 257 pp. Illus. Paper, $3.25.

Concealed under this book title is what is by all odds the best available general summary of information on the myriad of small islands, west of 180° longitude and mostly north of the equator in the Pacific, known as Micronesia. The emphasis is on geography, biogeography, ecology, and economic entomology, and a valuable gazetteer of place names is included. The islands covered are the Caroline, Marshall, Gilbert, Marianas, Volcano, and Bonin groups and the small isolated islands of Marcus, Wake, Ocean, and Nauru.

This is the first volume of a projected series on the insects of Micronesia and it is intended as a description of the geography and environment which may be used by the contributors to the remainder of the series, as well as by the general scientific public. The author, entomologist of the Bishop Museum, is director of a project to bring together and promote study of the enormous collections of insects from the Micronesian islands made by numerous investigators during and since World War II, as well as previous collections

by Japanese and others. This book summarizes much of the natural history and geographic information resulting from the many expeditions to Micronesia and the work of resident investigators there since the U.S. took over the islands at the end of the war. In addition, the author reads Japanese, which enables him to draw upon the extensive Japanese literature.

Sections on geology, soils, climate, flora, geography, fauna, ecology, economic entomology, as well as a list of principal collectors of insects and a gazetteer of place names, present in a brief space much of what is known for Micronesia in most of these fields. Some unpublished data were not available to the author, and some data he obtained from others were not as accurate nor as reliably interpreted as are his personal observations and researches. There are also many evidences of haste in certain parts, resulting from the necessity of making the work quickly available to the other collaborators in the series. Although this work would have unquestionably been better if written five years hence, after the accumulated collections have been identified and more information published, in its present form it is so much better than anything else, as a compendium of the geography and natural history of Micronesia, that criticism is scarcely in order. The volume can be freely commended to anyone interested in Micronesia or in the Pacific in general.

F. R. FOSBERG

Falls Church, Virginia

Die Evolution der Organismen. Ergebnisse und Probleme der Abstammungslehre. Gerhard Heberer, Ed. Gustav Fischer, Stuttgart, ed. 2, 1954. Part I, *Grundlagen und Methoden,* 176 pp. Illus. DM 14.30, subscription, DM 12.10. Part II, *Die Geschichte der Organismen,* 248 pp. Illus. DM 21; subscription, DM 17.70. Part III, *Die Kausalität der Phylogenie,* 288 pp. Illus. DM 23.90; subscription, DM 20.20.

The number of completely revised editions of works in the field of evolution that were first published only 10 years ago indicates the rapidity of the recent advance in evolutionary biology. This would seem to belie the opinion of some outsiders that evolutionary research had matured to the point of stagnation. The revised edition of Heberer's *Die Evolution der Organismen* is, in many respects, a new work. Five of the 19 contributors to the first edition of 1943 have dropped out and have been replaced by six new contributors. The new edition is published in installments, three of which have now appeared, all in 1954. Nearly all the chapters have been completely rewritten, and are organized into four major sections: (i) Principles and methods; (ii) the history of organisms; (iii) the causes of evolutionary change; and (iv) phylogeny of the hominids.

The following new or completely rewritten chapters are specially noteworthy: Rensch on phylogenetic changes of ontogeny, Lorenz on psychology and phylogeny, Remane on the phylogeny of animals, and Friedrich-Freksa on the evolutionary role of viruses and the problem of the origin of life. The other contributors are Dingler, Eickstedt, Gieseler, Heberer, Herre, Krogh, W. Lehmann, Lüers, Ludwig, Mägdefrau, Reche, Rüger, Schwanitz, Ulrich, Weigelt, and W. Zimmermann. Remane's contribution is a brilliant survey of the phylogeny of animals which, particularly with respect to the invertebrates, presents many original concepts and observations. The emphasis in much of the volume is on phylogeny, as is characteristic for the evolutionary literature on the continent, yet two large chapters on genetics and evolutionary research in plants (127 pp.) and animals (110 pp.) present a full summary of modern research in systematics, cytogenetics, and population genetics. The work is well printed and lavishly illustrated (250 figs. in the first three installments).

The volume will be particularly useful to those who are unable to keep up with the flood of original papers in the field of evolution. There is no other single volume in any language that treats the subject even nearly as comprehensively.

ERNST MAYR

Museum of Comparative Zoology, Harvard University

Earth Sciences

Nuclear Geology. A symposium on nuclear phenomena in the earth sciences. Henry Faul, Ed. Wiley, New York; Chapman & Hall, London, 1954. xvii + 414 pp. Illus. $7.

Nuclear physics has always had strict relations with geologic problems and the interaction between the two sciences has been a most fruitful one. Suffice it to quote the extremely important part played by mineralogical and geologic considerations in the discovery of radioactivity, and the establishments of an absolute time scale for geology.

In the last years the progress of nuclear science has been conspicuous and also the special branch of the geologic application has made great strides. No systematic review had been published for many years and these circumstances make the present book very timely. As usual, the fact that probably no author exists who has the necessary encyclopedic knowledge, *and* the time to write a book such as this, has made the cooperative form of authorship necessary.

The parts of the book are: (i) Fundamental considerations, instruments, and techniques of detection and measurement; (ii) uranium and thorium; (iii) the abundance of potassium; (iv) rare gases and fission in nature; (v) heat from radioactivity; (vi) radiation damage and energy storage; (vii) hydrocarbons formed by the effects of radioactivity and their role in the origin of petroleum; (viii) geophysical exploration by nuclear methods; (ix) determination of absolute age; and (x) the origin of the earth.

The authors, 26 in number, represent an extremely well qualified and authoritative group; but in spite of this, occasional errors have escaped their attention.

Good indexes help very much in consulting this

book which is really full of information in a wide field of subjects (to which the titles of the chapters do not do complete justice).

It is also clear that the field is in a phase of rapid development and that for each problem solved, a new and interesting one arises. This is one of the reasons why the reading of this book is so stimulating.

In addition to all this, the book makes good reading and will interest not only the specialists but also many scientists in the broad sense of the word. I passed several pleasant and instructive hours in its company and can recommend it, not only to the specialists who presumably must have it but also to scientifically minded persons at large.

EMILIO SEGRÈ

Department of Physics,
University of California, Berkeley

Some Fundamentals of Petroleum Geology. G. D. Hobson. Oxford Univ. Press, London–New York, 1954. x + 139 pp. Illus. $2.90.

As inferred by the title, this small volume presents a detailed discussion of a few selected topics and is not an over-all treatise of the subject of petroleum geology. The contents, which are divided into five chapters and three appendixes, are almost wholly concerned with the subjects of reservoir fluids, origin, migration and accumulation of oil, and reservoir pressures. The general presentation of the material is excellent and the author's deductive reasoning is stimulating in that he attempts to apply quantitative data to these selected topics. The chapter entitled "Migration and accumulation" was found to be especially well prepared, with a good discussion of such topics as inclined oil-water contacts, and with several original drawings.

In several instances Hobson refers to formation names or proper names in which no geographic location is given. Examples and references are in some cases omitted from discussions of considerable importance. A short appendix of definitions includes relatively well-known terms, such as permeability and porosity, but excludes less known terms such as Athy's compaction law. It is hoped that future editions will eliminate these few criticisms.

The book is recommended to general readers of reasonable scientific background as well as to students of petroleum geology of considerable advancement.

PAUL W. HUGHES

Department of Geology, University of Illinois

Applied Geophysics in the Search for Minerals. A. S. Eve and D. A. Keys. Cambridge Univ. Press, New York 22, ed. 4, 1954. x + 382 pp. Illus. $7.50.

The original edition of this well-known book appeared in 1929. With the smaller book by E. Pautsch, it represented one of the first few books written in North America relative to geophysical prospecting methods. As a text used at McGill University for more than 20 years, it is natural that it chiefly emphasizes geophysics in the search for ores. It presents the subject in terms of the specific geophysical methods, with emphasis on the instrumentation that has been used or is being used. There are 160 figures showing instruments, circuit diagrams, and results of field tests or surveys. A new chapter on radioactive methods has been added; also newly included is material concerning gravimeters and gravimeter surveys; seismic reflection methods; air-borne magnetic methods, and a brief description of Lundberg's air-borne inductive equipment. References to about 250 articles and books are included. The most conspicuous omission in this list of references, in my opinion, is the failure to cite the GSA special paper No. 36, "The handbook of physical constants of rocks and minerals," 1942, edited by Francis J. Birch. This book contains a wealth of data basic to geophysical prospecting methods.

The book is written with a contagious degree of enthusiasm for the subject and will be especially appreciated by the nonmathematical reader who desires a clear introduction to an inherently complicated subject.

LOUIS B. SLICHTER

Institute of Geophysics,
University of California, Los Angeles

Seismicity of the Earth and Associated Phenomena. B. Gutenberg and C. F. Richter. Princeton Univ. Press, Princeton, N.J., ed. 2, 1954. ix + 310 pp. Illus. $10.

The first edition was exhausted within five years. This proves that this book is of high quality and responds to a need. The second edition follows the same outline but has been brought up to date by addenda augmenting the bulk by 14 percent. Divided into three parts, the first part gives information on the material upon which the book is based and on the methods used in collecting and treating this material. A general outline on the seismicity of the earth is given, including discussions on the classification, frequency, and energy of earthquakes, as well as on our knowledge of the structure of the earth based on observation of earthquakes and interpretation of seismic records. Two world maps give the general geographic distribution of earthquake foci, showing the main belts on which the seismicity of the earth is chiefly concentrated.

The second part consists of a regional description and discussion of seismicity of the earth by text and maps, for which purpose the earth's surface has been subdivided into 51 regions. As pointed out, the work is intended "to discuss the geography and the geologic character of the zones and areas of seismic activity." This includes correlation with alignments of active volcanoes and gravity anomalies, and with oceanic deeps, mountain structures, and other topographic features.

Special sections are devoted to tsunamis (seismic

sea waves) and to a discussion of the causes of earthquakes and of the mechanism of focal motion. The second part ends with a list of references of 19 pages. The third part is a collection of chronologic and regional tables in which the damaging earthquakes since 1904 are listed. The earthquakes are classified by focal depth (shallow, intermediate, and deep shocks) and "magnitude." Information is given on time of occurrence, geographic coordinates of focus, focal depth, and the accuracy of this data. The "magnitude" of an earthquake is a very useful notion introduced in 1935 by C. F. Richter. It is a figure obtained from seismographic records, which is a measure of the energy released in an earthquake. A list of active volcanoes containing name, geographic coordinates, date, and character of the last eruption is included. The book is the standard work on seismicity of the earth. It will be a guide for anyone who wants information on this subject.

FRITZ GASSMANN

Institute for Geophysics,
Swiss Federal Institute of Technology

Climatic Atlas of the United States. Stephen Sargent Visher. Harvard Univ. Press, Cambridge, 1954. xii + 403 pp. Illus. $9.

The concept of climate is deceptively simple. Defined as average weather, or the long-term state of the atmosphere, depiction of climate might seem to be a simple matter of filling out a map with a few well-chosen symbols. Unfortunately (or perhaps fortunately, at least for the intellectually curious), climate contains so many facets that no stereotype has been agreed upon. In Visher's *Climatic Atlas of the United States* more than a thousand maps and diagrams are employed. As the exposition of climate by means of manifold maps is perhaps Visher's chief contribution to climatology, it is interesting to see how effective the technique is in the present volume.

The maps and diagrams are mostly simple, crisp line drawings. At a scale of about 1 : 40,000,000, three maps are placed on a page only slightly oversize (7 by 11½ in.) Legends appear in the margins and, with the exception of a short introductory text and a few pages of explanation elsewhere, the maps stand alone. Temperature and precipitation command the major part of the atlas (688 maps) but the remainder covers a wide range of subjects (not flying weather, however), including such disparate topics as ratings of regional climates as they, presumably, affect human energy, the annual death rate attributable to lightning, and the maximum depth that frost penetrates the ground (compiled originally by the U.S. Weather Bureau from reports of gravediggers). Usual runs of annual and monthly means are present in abundance, but weekly and seasonal periods are treated as well, and many terms other than the arithmetic means are shown.

In this large collection, most persons cannot fail to find some maps of real interest. The search for specific maps is aided by an index and by a reasonable arrangement of the subject matter. A cover-to-cover perusal of the atlas is a rather dull chore, however, leaving impressions of duplication (one subject, the frequency of heavy rains, appears in three closely similar maps) and of discrepancies in style and content.

Undoubtedly the greatest limitation was imposed by the choice of map scale. As examples of the generality of the maps, one notes that a climatic hint is lacking as to the presence of national forests in Nevada; even the outstanding economic importance of winter snow packs on western mountain ranges is poorly shown on maps of snowfall and snow cover. Diversity of subject rather than perfection of detail is the strong point of this work and, judged by the reception of climatic maps in the past, it is safe to predict that the *Climatic Atlas of the United States* will find a large use for many years.

HARRY P. BAILEY

Department of Geography,
University of California, Los Angeles

Medical Sciences

Handbook of Radiology. Russell H. Morgan and Kenneth E. Corrigan, Eds. Year Book Publ., Chicago, 1955. x + 518 pp. Illus. $10.

This *Handbook* is largely a compilation of a large amount of quantitative data related to the use of ionizing radiations in medical, scientific, and industrial applications. It should be of great value to workers in these fields, at levels of both basic research and practical application. The specific data here included in one volume are available elsewhere only from a variety of sources.

The material is divided into six major sections and four appendixes as follows: Definitions of physical terms and units, including conversion formulas and tables; general physical information (including biophysical data); radiotherapeutic data; radioisotopes; radiography and fluoroscopy; radiation protection; common drugs used in radiology; mathematical tables; the Greek alphabet, and schematic diagrams of x-ray generators and particle accelerators. There is an excellent and comprehensive index. Noteworthy for their inclusion are sections on medical radiographic technique, complete data on radioactive isotope physical characteristics and decay systems, and summaries of radiation protection material from the many handbooks published by the National Bureau of Standards for the National Committee on Radiation Protection.

The typography is legible and the proofs were evidently carefully corrected. Certain workers in medical radiations might have wished for additional radiobiological data, but the authors may have been unwilling to include material, the accuracy of which might not yet be firmly established.

This *Handbook* is authoritative, remarkably complete, handy in format, and well organized. It should

be an excellent reference work for all who deal with x-rays and radioactive materials.

RICHARD H. CHAMBERLAIN

School of Medicine and Graduate School of Medicine, University of Pennsylvania

Human Physiology. Bernardo A. Houssay, Juan T. Lewis, Oscar Orias, Eduardo Braun-Menendez, Enrique Hug, Virgilio G. Foglia, and Luis F. LeLoir. Trans. by Juan T. Lewis and Olive T. Lewis. McGraw-Hill, New York–London, ed. 2, 1955. xvi + 1177 pp. Illus. $12.

Bernardo A. Houssay, professor of physiology and Nobel prize winner in physiology and medicine, was assisted by six Argentine colleagues in physiology, biochemistry, and pharmacology, in writing this second edition of *Human Physiology*. There are 504 excellent illustrations, and a foreword by Herbert M. Evans of the University of California. The first edition of this superior textbook was translated into English in 1951. It has also been translated into French and Portuguese.

The book was written for undergraduate medical students and doctors of medicine. It can also be a valuable reference volume for all teachers and investigators in the biological sciences, as many life processes in all animal species have much in common. The authors of this superior book bring us up to date in the fields of contributions of animal experimentations to the understanding of man in health and in disease. The nine sections of the book are divided into 88 chapters. At the end of each chapter there is a brief, but up-to-date and challenging, bibliography of the main subject of each chapter. In producing this book our Argentine colleagues have rendered a significant service to biologic and medical education in many lands.

A. J. CARLSON

Department of Physiology, University of Chicago

British Pharmaceutical Codex, 1954. Pharmaceutical Press, London, 1954. xxxii + 1340 pp. £3 3/.

In the field of drug standards, a national pharmacopoeia implies a book of selective scope and legal force. Such a "pharmacist's bible" sometimes has been called a codex, as it is today in France. But the *British Pharmaceutical Codex* represents at a high level another type of pharmaceutical literature: reliable, general reference works supplementing the national pharmacopoeias.

The British *Codex* first appeared in 1907, by authority of th. Pharmaceutical Society of Great Britain. It gains added authority by the increasing reliance placed on it, by the courts and by administrators of the National Health Service, for standards covering drugs not included in the *British Pharmacopoeia*. This accounts for the increased attention given to standardization procedures and tests in preparing this fifth revision and its predecessor. The book has no exact parallel in America, although in outlook and practical effect it is roughly analogous to the *National Formulary*, issued by authority of the American Pharmaceutical Association.

The *Codex* consists mainly of six parts: (i) monographs on simple or basic drugs (842 pp), (ii) antiserums, vaccines, and related substances (50 pp.), (iii) preparations of human blood (12 pp.), (iv) surgical ligatures and sutures (2 pp.), (v) surgical dressings (40 pp.), and (vi) formulas for compound preparations (260 pp.). Monographs on many additional drugs (to list them would require two pages of small type) have been added to this edition, but about three times as many drugs have been deleted. The *Codex* provides three types of information that are subjects of consideration and debate among those responsible for the content of our *National Formulary*: antidotes for toxic substances, statements of action and use, and, in an accompanying pamphlet, trade names used for *Codex* substances or for preparations containing the substances.

There are eight appendixes of pharmaceutical chemical data and procedures and an excellent index.

GLENN SONNEDECKER

University of Wisconsin, School of Pharmacy

Connective Tissue in Health and Disease. G. Asboe-Hansen, Ed. Munksgaard, Copenhagen, 1954. 321 pp. Illus. $7.50.

During the past two decades there has been developing a many-sided and intense interest in human connective tissue. This is perhaps the most significant site in which to study the fundamental processes associated with an important group of human diseases. The interest has been stimulated by a series of recent discoveries and new concepts outstanding among which are the spreading factors by Duran-Reynals, hyaluronic acid and hyaluronidase by Karl Meyer, the recognition of mucoid swelling and fibrinoid degeneration as characteristic of several rheumatic diseases by Klinge, the focusing of attention on connective tissues in the broad group of "collagen" diseases by Klemperer, the electron microscopic pattern of collagen by Schmitt, and the clinical effect of cortisone in rheumatoid arthritis by Hench. Stimulation by these particular events, as well as by a climate favorable to medical research, has resulted in a broad attack on problems of connective tissue by every available technique from every possible point of view. Information has accumulated so rapidly in so many fields that it is difficult for individual workers to see the growing forest for the budding trees.

This volume is a survey of contemporary work and thought on connective tissue intended primarily for medical workers. There are 23 articles by as many authors, 19 of whom are from Denmark or the United States. The articles are almost equally divided among three groups. The first group, at a chemical and histological level, discusses morphology, histochemistry, ground substance components, collagen, mucopolysaccharide metabolism, sulfate exchange, and hyalur-

onidase inhibition. The second group, at a more physiological level, considers spreading, hormonal effects, aging, wound healing, pathology, the reticuloendothelial system, infection, and the influence of hormones on infection. The third group is concerned with diseases: cancer, arteriosclerosis, collagen diseases, arthritis, fibroses, and skin and eye diseases.

The articles are by workers well known in their fields and are generally well written, clear, and informative. As in any rapidly developing field, there must be latitude for differences of opinion. It might be easy to carp at the omission of some related fields of work but it would be difficult to produce a better concise summary for the purpose intended. The book is well printed and includes a reasonable number of references.

MAXWELL SCHUBERT

Study Group on Rheumatic Diseases and the Departments of Medicine and Chemistry, New York University College of Medicine

Biochemical Investigations in Diagnosis and Treatment. John D. N. Nabarro. Little, Brown, Boston, 1955. ix + 299 pp. Illus. + plate. $6.

The great acceleration in the application of biochemistry to clinical medicine that has occurred in the past 25 years has received increasing recognition in various reviews and textbooks. The author of the present book has not attempted any comprehensive survey of this field. Rather he has set himself the task of preparing a short, practical work that will serve the hospital resident in the biochemical aspects of the problems that are likely to be met on the hospital ward.

The chief concern of Nabarro is to show the way in which the results of various laboratory procedures may be utilized in the diagnosis of disease and the management of the patient. Some of the material has been arranged in accordance with clinical categories as, for example, Chapter VIII, "Diseases of the gastro-intestinal tract and pancreas" and Chapter IX, "Diseases of the liver and biliary tract," but most of the subject matter has been classified biochemically. Among the major topics that are discussed in individual chapters are disturbances of protein and nitrogent metabolism; disturbances of fat metabolism; disturbances of water and sodium metabolism; acid-base equilibrium; potassium, magnesium, iron, copper calcium and phosphorus metabolism; biochemical examination of the urine; the cerebrospinal fluid; the endocrine glands; the vitamins; and poisoning.

In general, little space is devoted to discussion of the nature of the biochemical derangements, although some chapters, such as those on disturbances of water and sodium metabolism and of acid-base equilibrium contain thoughtful and rather complete presentations of the subject matter. A number of recently developed procedures applicable to clinical medicine are described. These include paper electrophoretic patterns (p. 72) and tubeless gastric analysis (p. 126). However, in other instances as, for example, the consideration of phosphatase (p. 89) or diabetes mellitus (pp. 95–109), the limitations of space self-imposed by the author result in a discussion that, either in detail of information or analysis of the problem, is not much beyond that which the resident has probably acquired in medical school.

The absence of specific references to recent and current investigations in the literature contributes to the fluency of the presentation but does not give any lively sense of our changing views and present uncertainties about the biochemical mechanisms in a number of diseases. The author has failed to mention some interesting recent advances, such as the Coris' demonstration of the enzyme defects in glycogen storage disease, the variants of the hemoglobin molecule in the group of the hereditary anemias of the sickle cell, thalassemia and related types, or the evidence for the effect of insulin on cell permeability to glucose. This book would probably have gained considerably from the presentation of specific case material to illustrate the various biochemical defects.

Within the limits of his expressed aim, however, the author has fulfilled his purpose well. The general style and readability of the present volume should commend it to interns and residents in connection with the everyday problems met in their hospital training.

OSCAR BODANSKY

Sloan-Kettering Institute for Cancer Research, Memorial Center for Cancer and Allied Diseases

Pharmakotherapie des Fiebers und der fieberhaften Affektionen. R. Isenschmid, E. Glanzmann, H. Berger, and T. Gordonoff. Hans Huber, Bern–Stuttgart, 1954. ix + 360 pp. Illus. Dm 29.80.

Throughout the history of medicine, fever has been recognized as an expression of a pathologic process. *Calor* is one of the four cardinal symptoms in the classic, Celsus description of inflammation. Nevertheless, it was only 300 years ago that quinine, the first antipyretic, was discovered and it was not until the end of the 19th century that chemists were able to synthesize new medicaments capable of combating febrile diseases on an etiological basis. Today, the physician has at his disposal an entire arsenal of effective antipyretic medicaments.

This book deals with the problem of fever, febrile diseases, and the means of combating them. It is divided into three major sections. The first deals with the pathophysiology of fever. In this section Isenschmid discusses the regulation of body temperature, the changes of temperature that occur in disease, and the "utility" of fever. The second section handles the clinic of febrile affections. Glanzmann and Berger consider the normal regulation of body temperature, the causes of fever, different types of fever (transient, continuous, remittent, intermittent, recurrent), the significance of fever in differential diagnosis and prognosis, and finally fever therapy. The book concludes with a comprehensive section by Gordonoff on

the pharmacology and toxicology of antipyretic substances. In it he dwells especially on the dangers of these "vest pocket medicaments."

Aimed at the general physician, this book is solidly grounded in recent research. It is recommended to anyone concerned with the problem of fever and febrile diseases.

GEORGE ROSEN
School of Public Health, Columbia University

Animal Agents and Vectors of Human Disease. Ernest Carroll Faust. Lea & Febiger, Philadelphia, 1955. 660 pp. Illus. + plates. $9.75.

Although the words *Parasite* and *Parasitology* do not appear in the title, this latest textbook by Faust actually covers the field of parasitology. The book is new in the writing, in emphasis, and in organization. Its generic relationship to the books previously written by the author is revealed by the fact that 70 percent of the 216 figures are taken either from Craig and Faust's *Clinical Parasitology* or Faust's *Human Helminthology.* Its scope is approximately that of the former of these texts except for the addition of section 7.

Following a section on general information about and orientation to the field, there are four sections dealing with the protozoa and worms which cause human disease. The insects and other arthropods which act as agents and vectors of human disease are presented in one section. Section 7, which is new to this book, presents: (i) the coelenterates, echinoderms, and mollusks which are occasionally harmful to man through venenation; (ii) poisonous fishes; (iii) poisonous lizards and snakes; and (iv) harmful mammals. A final section deals with parasitological diagnosis and instructions for the collection and preservation of specimens.

The presentation is accurate, succinct, and readable. Coverage of subject matter over such a wide scope is accomplished at the sacrifice of adequate discussion of some of the more intricate or disputed subjects. Disproportionate space is devoted to the worms to the detriment of the protozoa and arthropods. The seven black-and-white drawings of malarial parasites original to this book will prove a poor substitute for the colored plates in *Clinical Parasitology.* The book is definitely better than a syllabus but falls short of being an exhaustive treatment.

C. G. HUFF
Parasitology Division,
Naval Medical Research Institute

Porphyrins. Their biological and chemical importance. A. Vannotti. Trans. by C. Rimington, Hilger & Watts, London, 1954. vii + 258 pp. Illus. + plates. 50s.

This book is a welcome and timely addition to the literature of the porphyrins. Whereas the volumes of Fischer and Orth consider the organic chemistry of the porphyrin compounds, and Lemberg and Legge's book is concerned with the formation and functioning of porphyrins from a biochemical point of view, this volume by Vannotti discusses normal and abnormal porphyrin metabolism from a clinical point of view.

The first half of the book includes chapters on some of the physical and chemical properties of the porphyrins, some methods on isolation of porphyrins from biological material, and a survey of the distribution of porphyrins in nature. The latter half of the book contains a valuable and extensive summary of the porphyrin diseases, their etiology, symptoms, and sequelae. Discussions and interpretations of these diseases presented here are no less valuable, even though they are admittedly speculative. The task of summarizing more than 500 references, especially to the older literature, was no light one and has been well done.

The present-day activity in the field of porphyrin biosynthesis is being accompanied by a lively interest in the human metabolic disorders—the porphyria diseases. In nature, the normal is often recognized by the presence of the abnormal. The understanding of normal biosynthetic pathways has often been aided by a study of mutants that occur in nature. Human pathology presents a wealth of data on mutants that represent a number of different kinds of porphyria diseases. This book contains a valuable summary of such diseases. It will be welcomed, not only by clinicians, but also by many of the workers in the field of porphyrin biochemistry who seek to understand the normal with the hope that they may alleviate the abnormal.

S. GRANICK
Rockefeller Institute for Medical Research

Perspectives in Physiology. Elza Veith, Ed. American Physiological Society, Washington 14, D.C., 1954. xi + 171 pp. Illus. $3.

When the triennial International Congress of Physiology last met (Sept. 1953, in Montreal), the International Council of Scientific Unions took the occasion to hold a Conference on the Future and Limitations of Physiology. Certain invited physiologists contributed brief discussions on what physiology meant to them and to their geographic groups (4 continents and 13 countries were represented). This small volume contains the more formal essays that they presented. Some authors talked about the subject matter of physiological science, some noted economic factors in its development, some dwelt upon historical personalities, and some presented their concepts of physiology's aims. All the essays will interest the general reader who may be curious about the ambitions of professional scientists, and will inform the scientific reader about numerous local scenes.

The volume records the current notions and hopes of physiologists. Some of the authors indulge in high thinking (Von Muralt, Homer Smith); others describe the vagaries of political and historical influences (Hoffman, Houssay). For some, history is still in the

making (Kuno, Rosenblueth); for others, history is in discontinuity (Bykov, Häusler). Physiology appears to lose ground where no public appreciates its worth.

The authors indicate that in most countries there is little or no physiology outside of medical institutions; actually, representatives of other brands of physiology were not heard from. All but two of the 17 contributors are shown in portrait; the portraits are inadequately reproduced. There is no index. Some errors of dates and spelling of proper names mar the book.

The general reader will correctly gather that physiologists around the world are factual folk who deal precisely with special and limited varieties of abstractions. Physiologists believe in laboratories, scientific lineage, instinctive behaviors, and what they are doing. They also have abiding faiths in a future that is socially hazardous.

E. F. ADOLPH

Department of Physiology, University of Rochester

Communications

Ten Commandments for Technical Writers

At the last session of the Conference on Scientific Editorial Problems at the AAAS Berkeley meeting, Elmer Shaw read these ten commandments for technical writers. He wrote them as a result of listening to the speakers in the first-day sessions.

1) Thou shalt remember thy readers all the days of thy life; for without readers thy words are as naught.
2) Thou shalt not forsake the time-honored virtue of simplicity.
3) Thou shalt not abuse the third person passive.
4) Thou shalt not dangle thy participles; neither shalt thou misplace thy modifiers.
5) Thou shalt not commit monotony.
6) Thou shalt not cloud thy message with a miasma of technical jargon.
7) Thou shalt not hide the fruits of thy research beneath excess verbiage; neither shalt thou obscure thy conclusions with vague generalities.
8) Thou shalt not resent helpful advice from thy editors, reviewers, and critics.
9) Thou shalt consider also the views of the layman, for his is an insight often unknown to technocrats.
10) Thou shalt write and *rewrite* without tiring, for such is the key to improvement.

ELMER W. SHAW

Rocky Mountain Forest and Range Experiment Station, U.S. Forest Service, Fort Collins, Colorado

That and *Which* Again

It was a pleasure to re-read the charmingly humorous Christmas piece reprinted from *The New Yorker* [*Science*, **120**, 7A (1954)]. But I have wondered again, as I did on first reading the article, whether the editor of *The New Yorker* seriously believed that the Bible could help us use *that* and *which* in the ways that would now be regarded as correct. And now I must also wonder whether the editors of *Science* believe it, since they tell us they reprinted the piece in the hope of softening the hearts of certain contributors who had been incensed at their "which hunting." Be that as it may, the reprinting seems to invite discussion of the use of *that* and *which* by Fowler, St. Matthew, and others.

The practice advocated by the Fowler brothers (joint authors of *The King's English*) is the most rational that has been described in print, and yet I believe it could be slightly improved. Their rule is, briefly: Use *that* for defining clauses, and *which* for nondefining clauses. They also appear to regard any clause properly begun with *that* as restrictive, and therefore not to be preceded by a comma, unless the comma be one of a pair enclosing a parenthesis ("the house, as you know, that Jack built"). They also appear to assume that any clause properly begun with *which* is supplemental, and therefore must be preceded by a comma. I do not think the correlation between pronoun and punctuation is quite so close, but in order to justify that view it is necessary to consider the difference in function between the two kinds of clauses.

Defining will serve to describe the proper function of a *that* clause, provided that we use the word in a rather broad sense. A *that* clause could rarely serve as a dictionary definition of the antecedent. What it generally does is to identify, or characterize, the antecedent by distinguishing it from other things (or rarely persons) of the same class. A clause properly begun with *that* can therefore be aptly described as a *distinguishing* clause.

Nondefining, the term the Fowlers apply to clauses properly begun with *which*, is not very useful, for it fails to tell us what such a clause does; it merely tells us one thing that it does not do. The main purpose of a *which* clause, it seems to me, should be to give us information about the antecedent, and a proper *which* clause may therefore be called an *informing* clause, or an *assertive* clause when that word better describes its tone.

Almost every clause properly begun with *that* is in fact restrictive, and therefore should not be preceded by an unpaired comma, but there are cases, though they are very rare, in which this rule does not seem

to hold. Consider the following speech from *She Stoops to Conquer*. Young Marlow's traveling companion is blaming him for their having lost the road:

> And all, Marlow, from that unaccountable reserve of yours, that would not let us inquire more frequently on the way.

The comma is necessitated by the presence of *your*, and it throws a fittingly ironical stress on *reserve*. But to use *which* in place of *that* would give the speech a smugly informative tone that would be quite out of key. The speaker is not informing Marlow of something that he knows only too well; he is characterizing his friend's "unaccountable reserve" by pointing to its deplorable result.

One would rarely, if ever, need to use a comma'd *that* clause. It is fairly often justifiable, however, to use a *which* clause that is not preceded by a comma—to use what I would call a "running *which*." A running-*which* clause may be to some extent distinguishing, but its dominant purpose is to inform or assert. This construction is most effective when the clause has an assertive, emotional tone, as in the following sentence from the Gettysburg address:

> It is rather for us, the living, to rededicate ourselves here to the unfinished work which those who fought here have thus far so nobly advanced.

A comma before *which* would not violate grammar, but it would slow down the impulsive movement which now sweeps through the sentence, and it would make the *which* clause sound rather dryly informative. To substitute *that* for *which* would inflict more subtle damage, by making the clause appear to be purely distinguishing—as if its purpose were merely to distinguish this unfinished work from some other unfinished work. The clause does of course do that, in a way, but its distinguishing function is of minor importance, for every intelligent reader knows at once what Lincoln meant by "the unfinished work." The main purpose of the clause was not to distinguish, nor was it merely to give us dry information; it was to pay a feeling tribute to the Union soldiers who had fought at Gettysburg. Its tone is emotional, or assertive, and it may be taken as a classic example of the assertive running-*which* clause.

The practice of St. Matthew in using the relative pronouns—or rather the practice of the translators who made the King James Version of the Bible—differed widely from that of the Fowlers, especially where the antecedent was personal. In a recent skimming of some dozens of pages, I nowhere found *who* used as a relative pronoun, though I did find one *whom* (II Kings 25:22). The translators most commonly used *that*, as in "Who is he that is born King of the Jews?" To use *that* with a personal antecedent, at least in a distinguishing clause, is still permissible, but even those who like the sentence just quoted as much as I do would not want always to use *that* to the exclusion of *who*. And it would not be even permissible now to use *which* with a personal antecedent, as is sometimes done in the Bible. In Chapter 7 of St. Luke, for example, we find "they which are gorgeously appareled," and "a woman in the city, which was a sinner." In two successive verses of St. Matthew himself, we find "unto him which hath" and "unto every one that hath." (Matthew 26: 28, 29).

With impersonal antecedents, the practice of the translators was not strikingly different from that of modern writers except in one respect: the translators apparently never used the compound relative *what*. The same sentence that contains the words "unto every one that hath" ends with the words "even that which he hath," whereas we would now write "even what he has." But the translators often used a *which* not coupled with *that* in a way that the Fowlers would not have approved, and I think they do so in the quoted sentence about the star (Matthew 2:2). The relative clause "which they saw in the east," placed as it is between paired commas, would appear to be giving us, parenthetically, a bit of new information. The fact it conveys, however, is not news, for we had recently been told (Matthew 2:2) that the wise men had "seen his star in the east." The clause is not informing but distinguishing; its purpose is to distinguish one bright particular star from all the lesser stars. It therefore should have begun with *that*; and there was no reason for putting commas around it.

If *which* were replaced by *that*, and three needless commas removed, the sentence would in my opinion be more logical and no less beautiful:

> And lo, the star that they saw in the east went before them, till it came and stood over where the young child was.

FRANK C. CALKINS

U.S. Geological Survey, Salt Lake City, Utah

25 January 1955.

Note on a New Literary Phenomenon

Americans are not a nation of readers; at least not readers of books. The American Institute of Public Opinion, reporting in 1954 on American habits and tastes, asked the question: Do you happen to be reading any book or books at the present time? Of the thousands queried, only 21 percent could or did answer affirmatively. But if few Americans read books they are at least definite concerning the kind of book they want them to be.

Publishers consider that any title that sells more than 100,000 copies in a year is a "best seller." By this standard the best-selling best seller in America today is a novel entitled *Not as a Stranger* written by the late Morton Thompson. This novel has been in print 2 years, and, while it is now down to seventh place on the *New York Times'* best-seller list, it remained No. 1 for more than 18 months. It has been through several editions, has been reprinted in a pocketbook edition, and is currently being made into a motion picture. It is estimated that some 5 million people have now read or are reading this book and

that it will remain on best-seller lists for at least another 2 years. The amazing fact about all this is that *Not as a Stranger* is not a love story, not an adventure study, not a murder mystery, not a popular psychology, not a comedy, not even a heart-thrilling historical novel. It is a thick book of more than 1000 pages about science and some of the men and women who devote their lives to science. It is a book about medicine, biology, physiology, and chemistry; about research and teaching and studying; about general practitioners, specialists, laboratory technicians, nurses, biologists, chemists, and physicists. It is concerned with the extent to which science compels idealism to compromise with reality and with whether, in a universe of pain and pretense, of atoms and absurdity, of fear and folly, there can be any place for a rational faith. It is not a "summer-afternoon-in-a-hammock" kind of book. It is big, vital, and provocative, carefully and accurately written, and its great popularity is the best evidence of the high place of scientific thought and activity in the literate man's present world.

Only a few places below it on best-seller lists is a factual book entitled *The World of Albert Schweitzer.* More than 100,000 people have each paid $5 to own this unusual collection of photographs and text about a most unusual man. He is not a young, handsome man, not a movie star, not a great sports figure; he is not the head of a nation, a titan in industry, or an eccentric multimillionaire. He is a man of science who is considered one of the greatest human beings of this century.

The Book-of-the-Month Club choices go monthly to more than a quarter of a million subscribers, thereby automatically insuring best-sellerdom to such choices. Its March 1955 selection is *Conquest of Man* by Paul Hermann, a German scholar. No novel, no informal, lightweight divertissement, this is a 455-page, $6 account of early discovery and exploration across the world. It is a book of archeology; it is history; it is science. The board of the Book-of-the-Month chooses its titles with at least half an eye to pleasing its vast membership. The choice of a lengthy book on archeology is strong evidence, again, of the mounting interest in things scientific on the part of the general reader.

Laura Fermi's *Atoms in the Family,* a biography of her late distinguished husband, Enrico Fermi, was chosen for condensation in *Omnibook* (March 1955). This roughly more than doubles the number of people who have already read it. Mrs. Fermi's account of the scientific achievements of her late husband is not at all technical or abstruse. This, however, does not lessen the significance of the fact that a biography of a nuclear physicist is being read by as many people as read a very good novel.

The only reason, apparently, that more science books, factual or fictional, are not on best-seller lists is that few such books have, as yet, been written. From the sales figures of the few mentioned here and from remembrance of such other best sellers as *The Sea Around Us, Annapurna, The Silent World,* and the like, it is obvious that books dealing with science or scientists or scientific data appeal not only to men of science themselves but to the general reader as well. The so-called "layman" is becoming more and more interested in such books. Writing them should prove not only challenging but rewarding to men of science. A genuine service would thus be rendered to a public eager to understand science and increasingly dependent upon that understanding almost for its very life.

AMELIA WEINBERG
1832 Biltmore Street, NW, Washington, D.C.

Do Ye unto Others

The editors of Science *make wide use of referees, sending nearly every paper that is submitted to at least one expert, and sometimes to two or three, in the appropriate field. In the following letter, one reader has set down his suggestions as to how these referees can most helpfully perform the sometimes arduous and annoying but always important task of reviewing manuscripts—a task for which they cannot be publicly thanked, but one for which the editors are constantly grateful, and by which the readers are regularly benefited.*

Refereeing a paper is a job that will interrupt your interesting work, occupy your valuable time, and bring you little or no credit or thanks. You might even be enthusiastically condemned. Why, then, should you bother to do it? Or, why should you bother to do it well?

Selfishly, you should appreciate this unique opportunity to become acquainted with some of the current work several months before it is published. At least a small part of it may be directly stimulating and helpful to you in your own thinking. Also, you should realize that others will be asked to give their valuable time to your own papers. Besides, is it not pleasant to have someone imply that your opinion is worth having?

Unselfishly, you should recognize that here is a chance to render worth-while assistance to progress in your field of interest. You may be able to provide real and valuable help to the author—help from which everyone may eventually profit. You are certainly placed in a position where you can aid the editors in their arduous and relatively thankless task of making the publication of greatest possible value.

Refereeing a paper is not just a chore, however, and not just an opportunity. It carries with it also a serious responsibility. Remember that as referee you are actually in a position of public trust. Not only the editor and the author, but also the public, are counting on you for a fair, thoughtful, and competent evaluation of the paper. Here are some suggestions that may be helpful.

The first point in thoughtfulness is to be reasonably prompt. It is all right to procrastinate in your own work, if you can get away with it, but please do not hold up the progress of science and frustrate someone else by sitting on his paper needlessly long.

You may not feel competent to judge a certain paper at all. Then why not return it immediately to the editor and tell him so? Or, you may feel that your competence in this field is limited. Perhaps you could comment to the best of your ability and also explain to the editor honestly what your limitations are with respect to evaluating the paper. No one knows everything, no one could possibly know everything, and certainly no one should be ashamed to admit not knowing everything, even within a highly specialized field.

You may find that a competent evaluation would require more time than you are prepared to give. If so, mention this fact to the editor with a tentative report, or return the paper without comment, explaining that it would require an unreasonable amount of time for one in your situation. Perhaps you can help the editor by suggesting someone else who could do the job more easily and better.

You may not understand part of the work described or may think it in error. Be sure that you have read exactly what the author *said* and not what you *expected* him to say. The most lucid exposition possible could make no dent on a tin ear or a closed mind.

If you think the paper too long for its content, try to help the author by suggesting specifically what he might condense or omit. Do not just tell him to give more data, expand the explanations, and cut the length to one-fourth. The author wrote it in the way that seemed best to him; if he is asked to revise the paper he should be given suggestions.

If you enjoy a wide reputation as an expert in the field, be *especially* cautious in what you say. The editor will value your opinion highly, so *be sure it is worth a high value.*

If you disagree with the author, *be specific* and cite book, chapter, and verse. The editor may accept you as an expert, but the author does not even know your identity. Certainly he is entitled to know the basis of your stated disagreement.

Publication of a poor or inaccurate or invalid paper is to be avoided if only because it wastes valuable space. Obviously it does not bring favorable notice to a journal; it may embarrass the editors and ought to embarrass the author. Nevertheless, probably no serious harm is done, since the readers most interested in the subject are usually reasonably skeptical and competent to judge. Therefore, as referee, beware of recommending against publication, *unless* you have every reason to be positive that you are *right and* are prepared to present the author with complete justification for your recommendation.

Remember that if you are a human being, scientific or not, you may be prejudiced against new ideas. In fact, you can hardly have become an expert without acquiring prejudice. Resist this prejudice! By approving a paper for publication, you are not espousing it—you are merely giving it an opportunity to be evaluated to the public. But by disapproving it for publication, you are assuming the far graver responsibility of depriving the public, without contest, of a fair chance to read and judge for itself. You become a self-appointed censor—are you positive you qualify? Certainly the repression of the truth would be a much more serious mistake than the publication of inaccuracies which can readily be checked.

Finally, if you can possibly find something good to say, please say it! Nonchemically speaking, a little sugar will help to neutralize a lot of vinegar.

In summary, when an editor sends you a paper to evaluate, imagine yourself in his position, forced to select critically from an overabundance of material, and write what he needs to know. *Then imagine yourself as author, and see how you would react to what you have written.* What better rule for referees than the Golden Rule?

R. T. Sanderson

Department of Chemistry, State University of Iowa

21 February 1955.

Achieving Style in Writing

The following gem of original natural-history observation is a 10-year-old's essay, "A bird and a beast," quoted by Ernest Gowers in his book, *Plain Words,* which was prepared for the guidance of British civil servants whose duties include tasks of writing.

"The bird that I am going to write about is the owl. The owl cannot see at all by day and at night is as blind as a bat.

"I do not know much about the owl, so I will go to the beast which I am going to choose. It is the cow. The cow is a mammal. It has six sides—right, left, an upper and below. At the back it has a tail on which hangs a brush. With this it sends the flies away so that they do not fall into the milk.

"The head is for the purpose of growing horns and so that the mouth can be somewhere. The horns are to butt with and the mouth is to moo with. Under the cow hangs the milk. It is arranged for milking. When people milk, the milk comes and there is never an end to the supply. How the cow does it I have not realized, but it makes more and more. The cow has a fine sense of smell; one can smell it far away. This is the reason for the fresh air in the country.

"The man cow is called an ox. It is not a mammal. The cow does not eat much, but what it eats it eats twice, so that it gets enough. When it is hungry it moos, and when it says nothing it is because it is all full up with grass."

Gowers commented: "The writer had something to say and said it as clearly as he could, and so has unconsciously achieved style."

SCIENCE is published weekly by the AAAS, 1515 Massachusetts Ave., NW, Washington 5, D. C. Entered at the Lancaster, Pa., Post Office as second class matter under the act of 3 March 1879. Annual subscriptions: $7.50; foreign postage, outside the Pan-American Union, $1; Canadian postage, 50¢.

SCIENCE

Publishing Scientific Books

ABOUT three-fourths of the books published by university presses receive one or another form of direct subsidy. This is a shocking figure. But even sadder is the fact that university presses must now sell from 2000 to 3000 copies of a book to break even.

There is little difficulty in publishing a book that will sell 5000 or more copies. But the average demand for highly specialized works, in science as in other disciplines, may be closer to 500 than to 5000 copies. The market for the serious book has never been large and is relatively inflexible. This has given rise to a large amount of literature on the publication problems of scholarly books. By and large, the bulk of this literature deals with the cost of manufacture. Although these costs are one important factor, they represent a relatively small portion of the retail price.

If we are to solve the problems of dissemination of specialized, scientific and technical information in book form, we shall have to devote more attention to factors other than manufacturing costs. The alternatives that have been tried, with varying success under varying conditions, include the following.

1) To increase the price—the usual German practice. We commonly pay as much as 7 to 9 cents a page for German scientific and technical works or $15 to $20 for a 200- to 300-page book. When this is done it is possible to reach a break-even point below 500 copies.

2) To increase sales. However, attempts in this direction indicate that the market for specialized works is quite inflexible, and these efforts have generally resulted in higher selling costs without a comparable increase in sales.

3) To lower manufacturing costs by use of other than conventional typesetting and printing processes. This includes the production of books by offset from typewritten copy and production by reduced facsimile (including microcard, microprint, microfilm, auxiliary publication in film form with an abstract printed in conventional form, and similar devices). This group of alternatives may be and has been helpful in some instances. Nevertheless, these efforts do not reduce marketing or overhead costs; and reduced facsimile, in particular, may increase these costs by restricting the potential market even below the normal one for specialized books.

4) To recognize that overhead and selling costs consume from two-thirds to three-fourths of the retail price of the book. Savings here may contribute more to the solution of the problem of publishing small editions than could the most sophisticated advances in typesetting or typesetting substitutes. In fact, if marketing were made automatic, and as few as 500 people wanted a book and were willing to pay for it in advance, the book (even including a reasonable number of illustrations and equations) could be set in type or in typewriter composition, printed on good book paper, bound in cloth, and delivered to readers at a cost of about 2 cents a page. If fewer than 500 wished copies, then the price would have to be increased somewhat; for printings of under 100 copies, reduced facsimile would be necessary.

It is not impossible to publish books that are needed. Proper design of the publishing operation is all that is required; but, in doing this, we must bear in mind that publishing is much more than printing.

If the market for a scientific book is 5000 or more copies, we have a remarkably efficient scientific book trade to handle the book. If the market is between 2000 to 5000 copies, the university presses can produce the book. If the market is below 2000, we have the choice of (i) a university press through subsidy; (ii) the commercial level, by increasing the price per page above that to which we have been accustomed from the American book trade; (iii) publication by specialized publishers who will give adequate thought to developing production *and marketing* methods to reduce overhead and selling costs. Finally, if an edition of fewer than 50 or 100 copies is needed, near-print or reduced facsimile methods coupled with automatic distribution can be utilized.

There is no necessity for us to be without scientific and technical books that make substantial contributions. The only problem is that of selecting or designing and applying the *most suitable* publishing procedure.

RALPH R. SHAW
Graduate School of Library Service,
Rutgers University, New Brunswick, New Jersey

SCIENCE, founded in 1880, is published each Friday by the American Association for the Advancement of Science at Business Press, Lancaster, Pa. SCIENCE is indexed in the *Reader's Guide to Periodical Literature.*

All correspondence should be addressed to SCIENCE, 1515 Massachusetts Ave., NW, Washington 5, D. C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts or for the opinions expressed by contributors.

Change of address: The notification should reach us four weeks in advance. If possible, please furnish an address stencil label from a recent issue. Be sure to give both old and new addresses, including postal zone numbers, if any.

Annual subscriptions: $7.50; foreign postage, outside the Pan-American Union, $1.00; Canadian postage, 50¢. Single copies 25¢. Special rates to members of the AAAS. **Cable address:** Advancesci Washington.

The AAAS also publishes THE SCIENTIFIC MONTHLY.

An Introductory Laboratory Course in Chemistry

By Howard L. Ritter. A manual designed to accompany the text, based on sound teaching principles and flexible enough to meet the needs of 3- or 6-hour courses at the freshman or sophomore level. Ready in April. Approx. 136 pages. Prob. $2.00.

Chemical Properties of Organic Compounds, An Introduction

By Elliot N. Marvell and Albert V. Logan, both of Oregon State College. Includes only those reactions most useful to non-major chemistry students. Discusses aliphatic and aromatic compounds separately, explaining the nature of each functional group. Ready in May. Approx. 300 pages. Prob. $4.75.

Biochemistry: An Introductory Textbook

By Felix Haurowitz, Indiana University. Emphasizes general principles of biochemistry and discusses several of the new biochemical ideas that have emerged during the last ten years. Includes information on the modern aspects of energetics and on the general occurrence of transfer reactions. Ready in May. Approx. 501 pages. Prob. $7.00.

Resonance in Organic Chemistry

By George W. Wheland, University of Chicago. Employing a uniformly chemical approach to the subject, the author deals explicitly with the applications of quantum-mechanical methods to problems of molecular structure. Includes an appendix on interatomic distances and bond angles. Ready in August. Approx. 830 pages. Prob. $12.00.

The Adams Symposium

Five papers given at the University of Illinois at a symposium held in honor of Roger Adams. The papers are: "Steric Effects in Dyes"; "The Structure of Gliotoxin"; "The Structure of Nepetalic Acid"; "Chemistry of Flavylium Salts"; and "Some Chemical Studies on Viruses." Ready in May. Approx. 128 pages. Prob. $3.50.

Androgens: Biochemistry, Physiology and Clinical Significance

By R. I. Dorfman, Boston University and R. A. Shipley, Western Reserve University. The only book devoted specifically to the subject. Compiles, summarizes, and evaluates the material. Ready in June. Approx. 556 pages. Prob. $7.00.

Electrochemistry in Biology and Medicine

Edited by Theodore Shedlovsky, Rockefeller Institute for Medical Research. Sponsored by The Electrochemical Society, Inc. Ranges from membranes and the application of electrochemical methods to problems of biological interest to the behavior of living tissues as electrochemical systems. Ready in May. Approx. 370 pages. Prob. $10.00.

Nuclear and Radiochemistry

By Gerhart Friedlander, Brookhaven National Laboratory, and J. W. Kennedy, Washington University. Covers the physical basis of the science of radioactivity, and includes thorough discussions of the preparation, properties, measurement, and applications of radioactive substances. Ready in August. Approx. 460 pages. Prob. $7.50.

Introduction to Chemical Pharmacology

By R. B. Barlow, Yale University. Shows the logical approaches to the design of new drugs and discusses the rational basis for the particular action and reaction of certain compounds. 1955. Approx. 330 pages. Prob. $6.25.

EARTH SCIENCES

Realms of Water

By Ph. H. Kuenen, University of Groningen, The Netherlands. Considers many aspects of physical geography, oceanography and meteorology, including data from a variety of sources. 1955. Approx. 320 pages. Prob. $5.25.

Send for on-approval copies.

440 Fourth Ave., New York 16, N. Y.

Meetings & Conferences

May

16–18. Radiation Research Soc., Hotel New Yorker, New York, N.Y.

16–19. American Assoc. of Cereal Chemists, St. Louis, Mo. (C. L. Brooke, Merck and Co., Inc., Rahway, N.J.)

16–20. National Conf. on Weights and Measures, 40th, Washington, D.C. (W. S. Bussey, National Bureau of Standards, Washington 25.)

18–20. American College of Cardiology, 4th annual, New York, N.Y. (P. Reichert, ACC, 140 W. 57 St., New York 19.)

18–20. European Assoc. of Exploration Geophysicists, 8th meeting, Paris. (Dr. B. Baars, 30 Carel van Bylandtlaan, The Hague.)

19. Maryland Acad. of Science, annual, Baltimore, Md. (J. W. Easter, Mt. Vernon Woodberry Mills, Mercantile Trust Bldg., Baltimore 2, Md.)

19–20. Soc. of Exploration Geophysicists, 10th annual Gulf Coast meeting, San Antonio, Tex. (SEG, 624 S. Cheyenne, Tulsa, Okla.)

23–25. American Trudeau Soc., Milwaukee, Wis. (Natl. Tuberculosis Assoc., 1790 Broadway, New York 19.)

23–26. International Surgical Cong., Geneva. (M. Thorek, 1516 Lake Shore Dr., Chicago, Ill.)

23–27. National Tuberculosis Assoc., annual, Milwaukee, Wis. (NTA, 1790 Broadway, New York 19, N.Y.)

26–27. American Statistical Assoc., Section on Physical and Engineering Sciences, New York, N.Y. (Jack Moshman, Bell Telephone Laboratories, Inc., Murray Hill, N.J.)

26–31. International Cong. of Comparative Pathology, Lausanne, Switzerland. (Prof. Hauduroy, 19 rue Cesar Roux, Lausanne.)

30–1. Chemical Inst. of Canada, Quebec. (Donald W. Emmerson, CIC, 18 Rideau St., Ottawa 2.)

30–3. International Hospital Federation, 9th cong., Lucerne, Switzerland. (J. E. Stone, 10 Old Jewry, London, E.C. 2, England.)

June

1–4. Alaskan Science Conf., 6th, College, Alaska. (Arthur S. Buswell, Box B, College.)

2–4. National Soc. of Professional Engineers, annual, Philadelphia, Pa. (K. E. Trombley, NSPE, 1121 15 St., NW, Washington 5.)

3–4. American Rheumatism Assoc., annual, Atlantic City. (W. H. Kammerer, 33 E. 61 St., New York 21.)

4–5. Soc. for Investigative Dermatology, annual, Atlantic City, N.J. (H. Beerman, 255 S 17 St., Philadelphia 3, Pa.)

5–10. American Soc. of Mechanical Engineers, Oil and Gas Power Conf., Washington, D.C. (C. E. Davies, ASME, 29 W. 39 St., New York 18, N.Y.)

5–11. Industrial Microbiology Inst., 8th annual, Lafayette, Ind. (C. L. Porter, Dept. of Biological Sciences, Purdue Univ., Lafayette, Ind.

6–10. American Medical Assoc., annual, Atlantic City, N.J. (G. F. Lull, 535 Dearborn St., Chicago 10, Ill.)

6–10. Industrial Research Conference, 6th annual, Harriman, N.Y. (R. T. Livingston, 409 Engineering, Columbia University, New York 27.)

6–11. World Congress of Anaesthesiologists, Scheveningen, Netherlands. (M. Mauve, Secretary General, Burger-Ziekenhuis, Amsterdam-Oost.)

PERSONNEL PLACEMENT

YOUR ad here reaches over 32,000 foremost scientists in the leading educational institutions, industrial laboratories, and research foundations in the U. S. and 76 foreign countries — at a very low cost

CLASSIFIED: 18¢ per word, minimum charge $3.60. Use of Box Number counts as 10 additional words.

DISPLAY: Rates listed below—no charge for Box Number. Monthly invoices will be sent on a charge account basis—provided that satisfactory credit is established.

Single insertion	**$19.50 per inch**
7 times in 1 year	**17.50 per inch**
13 times in 1 year	**16.00 per inch**
26 times in 1 year	**14.00 per inch**
52 times in 1 year	**12.50 per inch**

For PROOFS on display ads, copy must reach SCIENCE 4 weeks before date of issue (Friday of every week).

POSITIONS WANTED

Clinical Biochemist, Ph.D., director hospital laboratory eight years, blood grouping specialist, administration, publications. Box 88, SCIENCE. 4/1, 8, 15

Experienced Bacteriologist, Ph.D., wishes academic appointment in the S.W. Summer employment in West Coast or Rockies desired. Box 77, SCIENCE. X

Food Specialist; Ph.D., Foods–Nutrition expected May 55. Desire commercial or academic research. Any location. Box 116, SCIENCE. X

Internist, Board eligible; Ph.D. (Biochemistry); eight years' research experience in high blood pressure and other cardio-vascular renal diseases; particularly interested in directing clinical or basic research. Medical Bureau, (Burneice Larson, Director), Palmolive Building, Chicago. X

M.D. with research experience, publications etc. (endocrinology, circulation) seeking position in physiological, pharmacological research. Box 113, SCIENCE. X

Microbiologist. M.S. in mycology. Nine years experience in industrial fermentations (antibiotics, enzymes). Desires industrial research or college position. Box 108, SCIENCE. X

Microbiologist–Bacteriologist. Ph.D. with exceptional background and education and twenty years experience in academic, scientific and industrial research and leadership looks for responsible position where mature judgment counts: Professor, research–director, technical director, consultant, executive responsibility. Box 114, SCIENCE. X

Nutritionist–Biochemist; Ph.D., animal nutrition expected May 55. Desire commercial or academic research. Any location. Box 115, SCIENCE. X

Zoologist, Ph.D. experienced gross and microscopic autopsy laboratory animals, acute and chronic toxicity tests, histopathology, parasitology, desires position in research or commercial laboratory, preferably east of Mississippi. Available June 1. Box 117, SCIENCE. X

POSITIONS OPEN

Bacteriologist to head Chemotherapeutic Department of 20 in well-known eastern pharmaceutical company. At least 10 years of post-doctoral experience required with some knowledge of parasitology and virology. Employees know of this ad. Box 99, SCIENCE. 4/1, 8, 15, 22, 29

LOOK AROUND... notice the increasing number of satisfied advertisers using small-space ads regularly in practically every issue of SCIENCE—*proof that these small ads get results!* If you have a product or service of interest to scientists, take advantage of this low-cost means of increasing your profits!

POSITIONS OPEN

(a) **Head,** pharmacology department, university affiliated research institution; program calls for interest in chemotherapy of neoplastic disorders and metabolic antagonists; one oriented toward academic and investigative pharmacology required; East; $8–$10,500. (b) **Chairman** of chemistry department; associate or full professor; Women's college; Woman preferred; September. (c) **Biochemist;** Ph.D., experienced in pharmaceutical field; duties principally product development; East. (d) **Medical copywriter;** one of leading pharmaceutical companies; $8000–$8500. S4-3 Medical Bureau, (Burneice Larson, Director), Palmolive Building, Chicago. X

Opportunity is offered for college graduates of high standing to work toward the Ph.D. in Physiology. Background in chemistry and physics desirable. Tuition and modest financial assistance available to qualified applicants. Department of Physiology, Loyola University, 706 So. Wolcott, Chicago 12, Illinois. X

(a) **Organic Chemist;** Ph.D. initiate new basic research and supervise operation of research section; to $7000; resort community; East. (b) **Bacteriologist;** Ph.D.; faculty appointment; large university hospital; university city. (c) **Research Assistants** in Virology, Biochemistry, Pharmacology, Bacteriology; B.S.; salaries to $4500; noted pharmaceutical house. (d) **Microbiologist;** interested in virology; experienced various virus assays; duties to assay compounds and material for viricidal activities; M.S. preferred; New England. (e) **Biochemist;** M.S. or Ph.D.; male; consultant to staff members, also teach students in biochemistry; 350 bed hospital expanding to 500; Mid East. Woodward Medical Personnel Bureau, 185 N. Wabash, Chicago. X

Senior Research Biochemist—Ph.D. in biochemistry or physiological chemistry including broad background in basic medical sciences for east-coast Pharmaceutical Manufacturing Firm. Industrial experience helpful. Opportunity for eventual administrative duties. Send complete personal information. Box 86, SCIENCE. 4/1, 8, 15

Wanted: Biochemist, Ph.D. or equivalent, preferably an M.D., who would have complete direction of a large well-organized Anticoagulant Clinic, with the possibilities for research and would also be Director of the Bio-Chemical Department in the Clinical Laboratory. Box 112, SCIENCE. X

The MARKET PLACE

BOOKS · SERVICES · SUPPLIES · EQUIPMENT

CLASSIFIED: 25¢ per word, minimum charge $6.00. Use of Box Number counts as 10 additional words. Correct payment to SCIENCE must accompany ad.

DISPLAY: Rates listed below—no charge for Box Number. Monthly invoices will be sent on a charge account basis—provided that satisfactory credit is established.

Single insertion	**$19.50 per inch**
7 times in 1 year	**17.50 per inch**
13 times in 1 year	**16.00 per inch**
26 times in 1 year	**14.00 per inch**
52 times in 1 year	**12.50 per inch**

For PROOFS on display ads, copy must reach SCIENCE 4 weeks before date of issue (Friday of every week).

BOOKS AND MAGAZINES

WANTED TO PURCHASE . . .
SCIENTIFIC PERIODICALS and BOOKS

Sets and runs, foreign and domestic. Entire libraries and smaller collections wanted

WALTER J. JOHNSON • 125 East 23rd St., New York 10, N. Y.

The MARKET PLACE

BOOKS · SERVICES · SUPPLIES · EQUIPMENT

BOOKS AND MAGAZINES

BACK NUMBER PERIODICALS — Bought and Sold

• Tell us what you want?—What have you to offer?

Abrahams Magazine Service Established 1889 ▶ DEPT. P, 56 E. 13th ST. NEW YORK 3, N. Y.

Your sets and files of scientific journals are needed by our library and institutional customers. Please send us lists and description of periodical files you are willing to sell at high market prices. Write Dept. A3S, J. S. CANNER, INC. Boston 19, Massachusetts

---

TARGET: EARTH is collision geology. This book presents and evaluates evidence of repeated collisions of large meteors with the earth. Authors—Allan O. Kelly and Frank Dachille. 263 pages, $5.00. TARGET: EARTH, Box 335, Carlsbad, California.

FOR SALE

AMERICAN JOURNAL OF SURGERY, 39–73 bound, 74–88 unbound
ANNALS OF SURGERY, 101–122 bound
ARCHIVES OF SURGERY, 30–55 bound, 56–58 unbound
MILITARY SURGEON, 92–100 bound, 101–114 unbound, 3 issues missing
SURGERY, 3–19 bound, 20–35 unbound
BULLETIN OF WAR MEDICINE, 1–4 bound, 5–6 unbound
CANCER RESEARCH, 1–4 bound, 5 and 6 (1, 2, 3, 4) unbound
DIGEST OF TREATMENT, 1–8 partially bound, several issues missing

Contact: RESEARCH LIBRARY
WALLACE AND TIERNAN INCORPORATED
Box 178, Newark 1, New Jersey

The Identification of Crystalline Compounds

A detailed description of the technique for rapid identification of organic and inorganic crystals by means of the polarizing microscope is given in TECHNICS OF PLANT HISTOCHEMISTRY AND VIROLOGY, by T. E. Rawlins and Wm. N. Takahashi, University of California, Berkeley.

Price $3.50. Order from
THE NATIONAL PRESS, 435 Alma St., Palo Alto, Calif.

AVOID DELAY . . . . *Cash in Advance payment* MUST be enclosed with ALL classified ads. *See "Charges and Requirements"*

PROFESSIONAL SERVICES

BOOK MANUSCRIPTS INVITED

Write for Free Booklet SC telling how we can publish your book. All subjects considered. New authors welcome.

VANTAGE PRESS, Inc. • 120 W. 31 St., New York 1
In Calif.: 6253 Hollywood Blvd., Hollywood 28

PROFESSIONAL SERVICES

ANALYSIS OF AMINO ACIDS AND PROTEINS

VITAMINS, MINERALS, FOODS, SPECIAL DETERMINATIONS

SHANKMAN LABORATORIES
2023 SANTA FE AVENUE, LOS ANGELES 21, CALIFORNIA

WISCONSIN ALUMNI RESEARCH FOUNDATION

LABORATORY SERVICES

Project research and consultation in Biochemistry, Chemistry, Bacteriology and Entomology

Amino acid assays and biological protein evaluations • Vitamin and antibiotic assays • Chick feeding tests • Pharmacology including warm-blooded toxicity studies • Phenol coefficient determinations

Write for price schedule

WISCONSIN ALUMNI RESEARCH FOUNDATION
P. O. BOX 2059-V • MADISON 1, WISCONSIN

LaWall & Harrisson

Div. S, 1921 Walnut St., Philadelphia 3, Pa.

SINCE 1870

Food & Drug PROBLEMS

PHARMACOLOGICAL · BACTERIOLOGICAL · CHEMICAL

SUPPLIES AND EQUIPMENT

PARASITOLOGICAL PREPARATIONS

• high quality — low priced. *Write for catalog*

TROPICAL BIOLOGICALS • P.O. Box 2227, San Juan, Puerto Rico.

(THIS is the EXACT SIZE RECTANGLE on the Field Finder containing over 50000 lines and indicia. They form a simple, non-marking device for the microscopist to relocate fields of interest in a slide-mounted specimen. Other advantages: Superb precision—with interchangeability; non-destructive; used by substitution.)
WRITE FOR BULLETIN

LOVINSTRUMENTS
No. 201-050-150
Micro-Slide
FIELD FINDER
LOVINS ENGINEERING COMPANY
SILVER SPRING, MD.

"OUR PREVIOUS AD

created a great deal of interest, for which we are grateful. Additional advertising in your magazine is contemplated."

The MARKET PLACE

BOOKS • SERVICES • SUPPLIES • EQUIPMENT

SUPPLIES AND EQUIPMENT

SUPPLIES AND EQUIPMENT